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WHAT IS SOCIAL SCIENCE?

By Professor EDWIN B. WILSON

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LIKE many another English word, science is used in a variety of senses from knowledge or comprehension to art or skill and its meaning in any context must be judged from that context. The usual meaning seems to be knowledge gained by systematic observation or knowledge coordinated and systematized, *i.e.*, science, referring whether to method of gaining knowledge or to a body of knowledge, implies something more than sporadic activity. There is often contrast between science and art as may be illustrated by a quotation from Ruskin ("The Eagle's Nest"):

> In science you must not talk before you know. In art you must not talk before you do. In literature you must not talk before you think. . . . Science—the knowledge of things, whether Ideal or Substantial. Art—the modification of Substantial things by our Substantial Power. Literature—the modification of Ideal things by our Ideal Power.

It will be noted that Ruskin does not here emphasize the systematic nature of that knowledge which is science, but inasmuch as without a certain degree of continuity there could hardly be art or literature I take it that the passage must imply the same of science.[1]

[1] Whether it is justifiable to place this interpretation on Ruskin's words may be questioned. Certainly his primary definition of science is sheer factual knowledge. "It is not the arrangement of new systems, nor the discovery of new facts, which constitutes a man of science; but the submission to an eternal system; and the proper grasp of the facts already known." (Art. 37) "We are so much, by the chances of our time, accustomed to think of science as a process of discovery, that I am sure some of you must have been gravely disconcerted by my requesting, and will to-day be more disconcerted by my firmly recommending, you to use the word, and reserve the

There is often an antithesis drawn between pure and applied science of much the same import as that which Ruskin draws between science and art. The physicist is interested in physics as physics without regard to what he can do with that knowledge other than develop more physics; the engineer takes the physics and uses it for his engineering purpose. It is safe also to say that the engineer has to use something besides physics, he has to use his judgment in applying physics to his engineering purposes, and this means that he is in part at least an artist. In the same way medical practice has to be more than an application of unmodified medical science. Such a difference may be illustrated, perhaps with overemphasis, if one asks about the science versus the practice of billiards or golf. The science of golf is a matter of ballistics of an elastic body impelled by a blow from an elastic body and moving through an elastic and resisting medium to strike the earth and move on. This is a very complicated situation requiring for its discussion a deal of complicated mathematics, some of which was supplied by P. G. Tait, some more by subsequent writers, and some not yet available. It is not clear that the perusal of Tait's work could be of any assistance to a golfer in playing his game; indeed, I imagine that few golfers could follow Tait's theory and surely one would not require the science as a prerequisite to that practice by which the art of good golfing is acquired. Here the pure and applied sciences are probably, as in much of medicine and in some of engineering, largely dissociated; it is to be doubted if Tait became a better golfer by virtue of his profound studies, it is probable that the studies were suggested through his interest in the game and his intellectual curiosity to ascertain how much of the game he could explain by application of his mathematical technique.

thought of science for the acquaintance with things long since discovered, and established as true." (Art. 65) While such statements could be interpreted as limiting science as he uses it to facts, phrases like "proper grasp" may imply a considerable continuity or systematization of the knowledge. Further there are passages like that in Art. 134 in which Ruskin calls upon scientists to explain the formation of bubbles—something that really implies the inclusion of established theory as a part of our established knowledge. So, too: "Science does its duty, not in telling us the causes of sunspots; but in explaining to us the laws of our own life, and the consequences of their violation." (Art. 206) And theology is placed as the highest of the sciences: "The effect, for instance, upon your temper, intellect, and conduct during the day, of your going to chapel with or without belief in the efficacy of prayer, is just as much a subject of definite science, as the effect of your breakfast on the coats of your stomach." (Art. 67.) So far as concerns the artist in his work Ruskin specifically limits more sharply the meaning of science to facts concerning the looks of things: "I told you that I did not mean by 'science' such knowledge as that triangles on equal bases and between parallels are equal, but such knowledge as that the stars in Cassiopeia are in the form of a W." (Art. 124)

According to the "Century Dictionary" social science is:

> The science of all that relates to the social condition, the relations and the institutions which are involved in man's existence and his well-being as a member of an organized community. It concerns itself more specially with questions relating to the public health, education, labor, punishment of crime, reforming criminals, pauperism and the like. It thus deals with the effect of existing social forces and their result on the general well being of the community, without directly discussing or expounding the theories or examining the problems of sociology, of which it may be considered a branch.

Another dictionary might well give another definition. Some persons would say that the social sciences were economics, government or political science, history, sociology, social psychology and cultural anthropology—omitting education, public health, law, administration and other topics or perchance including them. Others might say that social science dealt scientifically with social situations, *i.e.*, with situations involving more than one person. It may be entertaining to note that according to the "Century Dictionary" economics is the "dismal science," which is annotated "humorous" without vouchsafing any information as to whether this humorous use of dismal refers to "gloom, melancholy, dolor" or to "a tract of land, swampy in character, often covered by a considerable thickness of half-decayed wood and saturated with water."

For whatever meaning may be attached to the word social science, it would seem that the significance of the noun science is at least as crucial as that of the adjective social; and as it has been seen that this noun is used in a great variety of senses and further as we are aware that most of whatever may be social science probably appears in written form, we may perhaps be permitted to examine the meaning of social science in the light of Ruskin's definitions: In science you must not talk before you know. In art you must not talk before you do. In literature you must not talk before you think.

Consider Plato's "Republic." The reason I invite your consideration of this work is because I rarely have attended a doctoral examination in the social science field but what the candidate has been asked questions respecting it. From this I infer that Plato's "Republic" must be a contribution of so great importance to social scientists that they expect their students to be familiar with it. This seems not to be true of the "Lamentations" of Jeremiah or of the "Revelation" according to St. John. How would the "Republic" classify under Ruskin's trilogy? Did Plato here talk before he knew? If so we can not rate it as science. How could he have known some of the things of which he talked—as, for example, of the communism of

property and of children among the ruling class? What criteria of knowledge could be applied of so biting a character as would drive other observers to draw the same inferences? As a matter of fact did not Aristotle draw quite the opposite inferences? And who shall decide and how shall he decide between Plato and Aristotle? In making a scientific decision one looks to the facts for enlightenment and for direction. It seems, at least to me, that we have to infer that Plato had not the facts, that he was expressing opinion about many matters on which it would be almost impossible to bring facts to bear—almost though conceivably not quite so difficult as to bring them to bear upon some of the matters in the "Revelation."

I infer therefore that Plato's "Republic" is not science. Is it art? That is, did Plato *do* before he talked? Had he an experience in social action whereby what he said could be rated fairly as a modification of substantial things by his substantial power? You may have information or inspiration on which you would answer: Yes. I have not and should be obliged to answer: No. On the other hand it does seem clear to me that Plato had thought before he wrote and that his "Republic" may well be characterized as a modification of ideal things by his ideal power and hence should be rated as a work in social literature. Indeed so great is the ideal power which shines out over the ideal things, so greatly are these things modified by that power, that none would hesitate ranking the work well toward the top of social literature.

We might turn next to F. S. Oliver's "The Endless Adventure" which is an account of Walpole's administration of the government of England. As I read this work it is a penetrating analysis of action—not Oliver's, to be sure, but Walpole's. In this respect the work differs from writings of Lord Haldane or of Lord Chesterfield on public administration, or of Chester Barnard on the function of the executive, all of whom actively engaged in administration; they did before they talked. However, we do not have much serious writing at an academic level by those active in public life or in business and I believe that in applying Ruskin's classification we should include in science, art and literature respectively the work of those who discuss the knowledge, action or thought of others. Where I asked above: Did Plato do before he talked, I had as lief ask: Had anybody done these things of which Plato talked? With this understanding we should classify Oliver's work as one of social art. In so doing I would not imply that Oliver does not propose some working hypotheses with respect to principles of action in the political field which if substantiated would be scientific laws of such action. Nor do I suggest that Oliver's work itself is not literature. All I am trying to do is to classify it essentially in the spirit of Ruskin's statement.

Let us try W. G. Sumner's "Folkways." As its name indicates this deals with the behavior of peoples. The author collected from many sources, compared and arranged for his undergraduate course a variety of folkways. He was not trying to show how various peoples ought to behave, but rather how they did behave and what values they held. The book should I think be rated as social science—at the descriptive or taxonomic level. It has not all the systematization or coordination which we might like to see applied to the field of its interest, but that is true of many a good text in science. It has not a theory attached to it as printed. Sumner once told me it was a case book and that if he lived long enough he might follow it with a theory of society. Theories that precede cases like Plato's "Republic" are very likely to be literature, a collection of cases that precedes theory may well rate as scientific data. Herbert Spencer had an evolutionary theory of society which he tried to substantiate to some degree with facts, but it is my belief that to-day most sociologists regard the known facts as not competent to prove the evolutionary theory of society in any such sense as the known biological facts prove the theory of biological evolution.

The literary tradition in the social sciences is very strong. By this I most certainly do not mean the tradition of fine literature. Those who practice the literary tradition may be very indifferent writers and very indifferent thinkers—they are merely those who deal with modifications of ideal things by their ideal power, however limited, with no particular control over those processes by facts, indulging in lengthy dialectic, often extremely clever, without starting from nor ever coming to statements which could be controlled by facts. Indeed unless I am much mistaken, so great is the prestige of this sort of thinking and writing on the part of those who are in a position to award recognition for work in social lines that many a young fellow might better imitate Plato's "Republic" badly than Sumner's "Folkways" well; yet if we are to advance scientifically in the narrow sense it would more likely be through constant revision, systematization and extension of the "Folkways."

Not long ago I was sitting with a group of thoughtful persons, mostly educators, when one of them asked another: What do people up your way think of the present European situation? His reply was: They don't think, they merely *emote*. I know of the transitive verb *emove*, though it is rare; I can find little justification for the intransitive *emote*, but it seems to be coming sparingly into use and perchance before long it will be in good usage. We need some word for abandoning one's self to one's emotions, for this is a common phenomenon. Many is the time that one does not know, nor do, nor think, but does *emote*. Maybe it should not be so. Maybe in those quiet

Victorian days Ruskin would not have done so nor admitted that any English gentleman would do so. The forebrain was then supposed to have ascendency over the midbrain and passion to be thereby restrained. Still this may have been a condition contrary to fact. Persons were supposed to be logical, but non-logical actions were then presumably very common as they are now. If in science one does not talk until he knows, in art until he does and in literature until he thinks, in what shall we say it is that one does not talk until he *emotes*? We need a word for that something, and I daresay the Greeks may not have had a name for it.

Consider Marx's "Communist Manifesto" written when he was twenty-nine. I shall not cite his "Kapital" which appeared twenty years later. The "Manifesto" is a diatribe against the bourgeoisie. However much it may be founded on knowledge, action or thought, its form and spirit appear to me to be in the same class as the hellfire and damnation sermons which are said to have been frequent right here in New England in earlier days; it is a work of a modern Hebrew prophet calling upon the people to renounce Baal and his idolatries and follow only the true Jehovah of whom Marx offers himself as the true prophet. It is shot through with emotion and is an appeal to emotions. And when we use the term emotion here we are not thinking of the rich sensuousness of art, nor of the strong sentiment of literature, and still less are we thinking of the intellectual estheticism of science; we are in the presence of hate and perchance of rage—as of one who hates the devil and rages against all his works. Where would Ruskin place the "Manifesto"? Where would he place "Nine long Harvards and three times three" which ring out these Autumn Saturday afternoons? So far as I gather the meaning of the "Eagle's Nest," I judge he had small place for them.

Just as we may need the word *emote* to signify abandonment to emotional reactions so we may need a word to denote those verbal productions which come direct from emotion and are intended to appeal directly to the emotions of the hearer or reader and thereby to incite him by sympathy to action. Shall we call them slogans or emotives or propaganda? Shall we supplement Ruskin and say: In propaganda you must not talk before you emote? At any rate, whatever be the terminology adopted I think we must readily agree that there is here something important which should be pointed out. In particular it should be pointed out for the purposes of this discussion of "What is Social Science" because it is surely true that the phrase social science as used to-day covers not only Ruskin's three gentlemanly categories but this fourth which we have been trying to portray and which may be socially more influential than the other three put together in about that proportion by which the proletarians exceed the bourgeoisie in the population. And shall we add to the definition of Art as the modification of Substantial things by our Substantial Power and to that of Literature as the modification of Ideal things by our Ideal Power a definition of propaganda as the modification of Substantial things by our Ideal Power, which may well be a polite characterization of lies, or as the modification of Ideal things by our Substantial Power, which might be a fair definition of forcible indoctrination?

If we use science in the strict sense of knowledge systematized and coordinated and generally accepted by professionally competent persons we have, I fear, to admit that there is very little social science. Many a time I have sat at the Social Science Research Council or at committee meetings at Harvard or elsewhere, when in groups of social scientists the conversation has turned to questions of their science. Under such circumstances one generally listens to disputes between different opinions which those participating hold and try to substantiate, each against the others, by appeal to reason or argument rather than by appeal to facts. If perchance somebody should point out that among natural scientists there would be more general agreement, the answer comes back that the questions under discussion are on the border line of research and that agreement should not be expected any more than a few years ago agreement on the meaning of experiments on cosmic rays was attainable as between Millikan and Arthur Compton or agreement as to the expanding, contracting, vibrating or stationary universe could be had between astronomers. It is of course a just observation that we do not expect agreement at the research line. If, however, one counters this observation by asking what it is that is known scientifically in the social field, the reply is something vague—such as that "there is a great body of economic science on which all or most economists are in agreement as is true of physics." When, however, one gets down to brass tacks and presses for the specification of something agreed upon from out of this great body of economic knowledge, one finds hesitancy; and if by an excess of daring some member of the group ventures some definite proposition as known, his confrères cry out that they do not agree. Statistically speaking, I have seen this sort of thing happen often enough under serious circumstances and have seen the contrary, namely, the successful specification of scientific knowledge, occur so infrequently that I am led to my inference that there is not much that is social science. There are, to be sure, as always, possible avenues of escape from such a particular inference; one might, for example, conclude that the social scientist is simply not interested in whatever it is that is known.

To me this would not be so ominous a situation were

it not that some of those least able or willing to concentrate on the scientific content of their science are among those most actively advocating social and economic control. Now while it is true that we have had and still have some empirical forms of materialistic control without a sound basis in natural science we have come from experience to know that such empirical control is validated only when it has been tried and has been observed to work time and time again so that each particular realized form of such control has attained that kind of uniformity of success which itself represents a scientific fact, and we do not expect a new attempt to be successful unless it is but a very slight modification of an old form; but on the other hand from the rapid and successful advances of the past century we have acquired the belief that the surest and safest way to gain new forms of control is through basing them soundly on ascertained science. How the situation in the social field can be different is difficult to see; those who advocate new types of social control may advocate them as experiments (though they rarely do, for they generally advocate them as rights or duties), but must be prepared for serious failure to achieve ends and even for the appearance of tragedies—unless and until a social science has been developed from which the results of the experiment can be predicted with assurance.

Turning now from social science in the strictly scientific sense we find a great deal of social science of the literary type, of which some might be converted into true science by careful investigation but of which much seems not to be so formulated as to admit of this conversion. And there is also a great deal of social art or action which might be so subjected to study as to yield accredited science. A scientist is a fellow who has technical competence to give a proof which is valid. In this connection I would refer to Chester Barnard who writes:[2]

> Whether the present essay is a contribution to the science hoped for remains to be determined by others. What has been presented is a hypothetical scheme which at present explains roughly to me what I have observed in many years of practical work with organizations of various kinds and what I have constructed from the experience of others, supplemented, of course, by a little knowledge of the social sciences. It is not the work of a scientist or scholar, but rather of an interested student of affairs.
>
> For this reason perhaps its chief value, if presently it has any, will merely lie in its expression of one view of experience. By it I have at least submitted my mental processes in this field to inspection. If it has any further value it will lie in the suggestion it may give to more competent inquiry, which I hope can be undertaken.

The work may not be science, the author does not rate it as such and I have ventured to follow him in this; but it is written more in the scientific spirit than many a work by a professional social scientist and is, I believe, in such form that it can be put to the tests that will convert it into science. Are there persons who have the technique and the interest to take up the author's invitation?

When my father was a student at Yale College, 1861–1865, the curriculum was largely classical. Greek, Latin, mathematics and rhetoric were the mental pabulum of the first two years. In the third year they continued with the addition of some natural science which went on into the senior year. The only subjects which could be considered as social science were in this last year. Here we find in the first of the three terms:

> History and political philosophy—Guizot's "History of Civilization," political economy begun, 4 lectures a week for 7 weeks by Pres. Woolsey;
>
> Mental philosophy—Hamilton's "Metaphysics," 4 lectures a week for 7 weeks by Prof. Porter, later president.

In the second term we find:

> History and political philosophy—Political Economy finished, Lieber's "Civil Liberty and Self Government," 4 lectures a week for 7 weeks by Pres. Woolsey;
>
> Moral philosophy—Stewart's "Active and Moral Powers," Butler's "Sermons," Hopkins' "Moral Science," 4 lectures a week for 7 weeks by Prof. Porter;
>
> Theology—Paley's "Natural Theology," Butler's "Analogy."

In the final Senior term:

> Political philosophy—Law of Nations;
> Constitution of the United States;
> Theology—Paley's "Evidences of Christianity."

I cite this program as indicating the climax of college education in those days at what was undoubtedly one of our best colleges. The catalogue listed no professor of social science—the President took this rôle—and presumably very ably, he was a real man. Porter was professor of moral philosophy and metaphysics; he was also in the faculty of divinity. W. B. Clarke who taught theology to the seniors was professor of divinity but his name does not appear in the list of that faculty, only in the list of the faculty of the academical department. Dutton who gave the fourteen lectures on the Constitution was from the faculty of law and his name does not appear among those of the faculty of the academical department. The point which I would make is that the social and moral sciences or philosophies were apparently treated along with theology as comparable and culminating parts of a liberal education. There is little likelihood that the scientific aspects of any of them were emphasized but the maturity and caliber of the men who gave them and the high standard of the texts which were used is adequate evidence that they were good liberal education. Moreover, when estimating what the students on

[2] "Function of the Executive," p. 292.

graduation knew about man one must take into account the tradition of education in the classics at that time which was to give attention to the life and ideas of ancient times—the reading of Thucydides was not just an exercise in the Greek language or literature but one in the interpretation of history.

The situation of the social subjects in the colleges to-day is very different. There are numerous courses and a large staff. The increase in the time given by undergraduates to the social studies has been very great, until at the present time the concentration in these fields amounts in some colleges to about one half of the whole. There is great attention to current and recent problems whereas as of my father's time there seems to have been relatively little. That those of our students who concentrate to-day in the social fields learn far more of their detail than students learned seventy-five years ago is certain, but it seems equally certain that those who concentrate in other fields by the very intensity of their concentration may have a far less philosophic outlook upon society than did all the students of those days. What the place of the social studies is or should be in present collegiate education I am not prepared at this time to discuss; were I to discuss it I should probably make some plea for a greater attention to law, education, public health, and possibly administration difficult though that subject is, than is usually accorded to them in such discussions. Furthermore, no matter how much I would emphasize the necessity for professionals to lay more stress on the strictly scientific aspects of social science or however much I might urge that even the collegian might be shown what is definitely known, I should be likely to suggest that where so little is known it would be well to consider the social sciences not as individual branches of learning but as an interest in man, reaching out even to theology as they did at Yale—for who shall assert that man lives by bread alone or that an ideology that triumphs over material conditions does not have to approach to a religion?

ISOSTATIC CONTROL OF FLUCTUATIONS OF SEA LEVEL

By Professor ANDREW C. LAWSON

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WITH the last retreat of the North American continental ice sheets the northerly drainage of the land was impounded by the ice front at various stages till, with the vanishing of the ice, it finally flowed freely to the ocean. The lakes so formed waxed in size as the ice front receded; but the only escape for their waters was to the south, over cols in the continental divide. There were many of these lakes, large and small, each with its own level of overflow, which remained approximately constant as the lake expanded. The shores of all of them, as they came against the emerging land slope of their respective basins, carved or built shore features, which are in most cases perfectly preserved to this day. At the time of their formation these shore features were of course level, but to-day they are for long stretches inclined to the horizon, up to the north. This inclination of the once level strand lines is due to a tilting of the surface of the earth, and affords an angular measure of the tilt, which is usually expressed in inches or feet per mile. The constant display of tilt up to the north, on the periphery of basins vacated by continental ice sheets to form lakes, has suggested to geologists that the rise is due to relief of load, consequent on the vanishing of the ice and the complete or partial draining of the lakes. This belief is now very generally held. It means that geologists have accepted the view that the earth's crust may be depressed by a local load upon its surface, and recover from the depression when the load is removed. That is, they have accepted isostasy as a valid doctrine. The phenomena of regularly titlted shore lines are perhaps the most impressive and convincing proof of the truth of that doctrine.

Isostasy means equality of mass, per unit of horizontal cross section, in all large vertical columns of the earth. The imposition in the Pleistocene of a broad load of ice, let us say one kilometer thick, upon a portion of the earth's surface threw the column carrying that load out of balance with the rest of the earth. The ice mass came from the ocean; and the column, the top of which is the sea surface, lost load, and was thus also thrown out of balance with the rest of the earth. The unglaciated portions of the continents were neither loaded nor relieved of load, but in glacial time they were nevertheless also temporarily out of balance with both glaciated and oceanic regions, in different degree. The whole disturbance was caused primarily by the transfer of load from the ocean to the glaciated region. If a load equivalent to that of the ice had been shifted in depth from below the ice to the region below the ocean, with uniform distribution as to both removal and addition, balance would have been restored over the whole earth. Since the gravitative tendency to restore isostatic balance, however disturbed, is always active, it is presumed that continental glaciation actually induced the shift of mass in depth from the loaded continental column to the un-

loaded oceanic column. The volume of the rock mass, thus removed in depth from the glaciated continental column, was less than that of the ice since its density is greater. Volume may be spoken of in terms of thickness, the layers we have to deal with being uniformly thick. Thus the thickness of 1 km of ice of density .917 added to the surface would be compensated by the removal in depth of .278 km of rock of density 3.3 and its distribution to other columns of less mass.

The compensating movement would have set in long before the full accumulation of the ice load, and it would at first have been directed toward both the ocean and the unglaciated portions of the continents. But as compensation for the full load advanced toward completion, the shift of mass in depth would have been from the continents as a whole to the oceanic column, till world-wide balance was re-established. At this stage, assuming that the full ice load continued for a time, the sea level, as compared with its pre-glacial position, would have been lower by the thickness of the layer of water removed to form the glaciers, less the rise of the sea bottom due to the invasion of the column in depth by heavy rock for compensation.

At this stage of universal balance, the surface of the unglaciated continental column would have had the same position, with reference to the center of the earth, that it had in pre-glacial time. The surface of the glaciated column would have been higher and, as already stated, the surface of the oceanic column lower. The mass added to the oceanic column would have been distributed over its entire area, as was the water that went to form the ice sheets. It is difficult to imagine any method of distribution of rock mass from the continental columns to the oceanic column other than plastic flow. The transfer of mass does not imply that the same rock taken from the continental column was delivered to the oceanic column, but only that equality of mass in the two columns was re-established by plastic adjustment.

The problem of how plastic shift of mass in depth, induced by shift of load at the surface, affects the level of the ocean involves the use of data which are not yet fully or certainly determined. Thus the density of the heavy rock, which in depth compensates the oceanic column for loss or gain of sea water, is but vaguely known. Its specific gravity is at least 3.3, and this value is used in the calculations which follow. The arithmetical value arrived at for the fluctuation of sea level lacks precision, but that the fluctuation is real and is isostatically controlled can scarcely be questioned.

An approximation to the mean thickness of the Pleistocene ice sheets at their maximum may be obtained by considering the Greenland sheet of to-day. Lauge Koch gives us a map[1] of Greenland with a few contours on the surface of the ice. The average gradient of this surface, measured on six different lines between the 3,000 m and the 2,000 m contours, is 1 in 459. But the ice front except in the fjords moves very slowly, if at all, and Coleman[2] cites Koch to the effect that it is about stationary.

The Labrador ice sheet flowed from its center of dispersion to and beyond the mouth of the Hudson River. H. L. Fairchild[3] places the maximum post-glacial uplift at Lat. 50°, Long. 75°. But A. P. Low[4] located the center of dispersion of the ice between Lat. 53° and 55°. A probable position for the center is thus Lat. 54°, Long. 75°. The distance from this point to the mouth of the Hudson River is 1,500 km. The present altitude of this center is not precisely known. But Low gives the altitude of Lake Mistassini as 1,300 feet. The mean altitude of the region around the lake is about 300 feet[5] above its surface. This gives us 1,600 feet or about .5 km above present sea level.

When the front of the Labrador sheet, in the wane of the latter, withdrew from the ocean at the mouth of the Hudson River, it occupied a succession of positions in its retreat, at each of which waste just balanced the forward flow of the ice. One of these positions was the present shore line at New York. Then, considering the front as stationary for a brief time, the gradient of the surface of the ice was probably about the same as that of the Greenland sheet to-day, 1:459. Then the ice surface at the center of dispersion would have been $\frac{1}{459} \times 1{,}500 = 3.27$ km higher than at the front. We may suppose that at this stage the surface of the Labrador sheet at its front was .3 km above present sea level, and that at this, or any other stage of the early retreat, the thickness of the ice at the front was also .3 km. Then the center of dispersion was $3.27 + .3 = 3.57$ km above the contour of the present shore. The load of ice isostatically displaced an equal mass of heavy rock in depth, having a specific gravity assumed to be 3.3; and there was a depression of the region corresponding to the thickness of the heavy rock displaced. If x be the thickness of the ice at the center of dispersion, $x = 3.57 - .5 + \frac{.917x}{3.3}$, whence $x = 4.3$ km; and the mean thickness of the ice from the center of dispersion to the mouth of the Hudson River was about $\frac{4.4 + .3}{2} = 2.35$ km.

[1] *Jour. Geol.*, 31: 48, 1923. Reproduced by Coleman in his "Ice Ages Recent and Ancient," p. 29, 1926.
[2] *Op. cit.*, p. 28.
[3] *Bull. Geol. Soc. Amer.*, 29: 187–234, 1918.
[4] *Geol. Survey Can. Ann. Rpt.*, viii, p. 387 L., 1897.
[5] A. P. Low, *op. cit.*, p. 68 L.

But this thickness is that of a sheet having the same gradient as the Greenland sheet, or 1:459; and the front of the Greenland sheet is stationary. For maximum vigor of both Greenland and Labrador sheets, the supposition may be made that the mean thickness was 50 per cent. greater than that which corresponds to a gradient of 1:459. On this assumption the Labrador sheet at its maximum would have had a mean thickness of $2.35 \times 1.5 = 3.52$ km. If now, for the maximum vigor of the Labrador sheet, x be the thickness of ice at the center of dispersion and .3 km be taken as the thickness at the front, out in the Atlantic, then the mean thickness is $\frac{x+.3}{2}$. Thus, $\frac{x+.3}{2} = 3.52$, whence $x = 6.74$ km. For this load the isostatic depression was $\frac{6.74 \times .917}{3.3} = 1.87$ km; and the altitude of the ice surface at the center of dispersion was $6.74 + .5 - 2.04 = 5.2$ km. The gradient of the Labrador sheet at this maximum stage depends on how far the front had advanced beyond the line of the present shore.

If the mean thickness of the Labrador sheet at its maximum be assumed to have been 3.52 km, we may, to extend the discussion, adopt this figure for the mean thickness of all continental ice sheets at the height of pleistocene glaciation. Coleman[6] gives the area covered by continental ice in Pleistocene time as 12,000,000 square miles or 31,000,000 square kilometers. The volume of this ice is $31{,}000{,}000 \times 3.52 = 109{,}120{,}000$ cubic kilometers. To supply this ice a layer of water $\frac{109.12 \times .917}{368} = .27$ km thick was removed from the ocean, the extent of the ocean being 368,000,000 sq. km and the expansion of water on freezing $\frac{1}{12}$. The removal of this load of water would have necessitated the addition in depth of a layer of heavy rock, of specific gravity, say 3.3, for isostatic compensation. This layer of rock added to the oceanic column would have had a thickness of $\frac{.27}{3.3} = .08$ km, and would have raised the sea bottom that much. The net fall of sea level, relatively to the center of the earth, or to the pre-glacial sea level, was $.27 - .08 = .19$ km. The layer of rock added to the oceanic column had the same mass and the same volume as that forced out of the continental columns to compensate the load of ice.

Of the total area (31,000,000 sq. km) covered by ice sheets in the Pleistocene one half has been completely deglaciated, and the ice still remaining on the other half, comprising Antarctica and Greenland, has been reduced in thickness. The reduction may be again taken, for the purposes of this discussion, at about one third of the maximum. That is, the mean thickness of existing ice sheets, covering an area of 15,500,000 sq. km, is supposed to be 2.35 km instead of the 3.52 km at maximum glaciation. The total volume of ice thus restored to the ocean since the maximum Pleistocene glaciation is about 72,740,000 cu. km. As water spread over the ocean it is a layer $\frac{72.74 \times .917}{368} = .18$ km thick; and if no other movement occurred the sea level would be raised that much. But this load of .18 km of water has been, or is in process of being, compensated by the removal from the oceanic column in depth of a layer of heavy rock. The thickness of this layer, at the assumed density of 3.3, is $\frac{.18}{3.3} = .05$ km; and the sea bottom would subside that much. Thus the net rise of sea level due to the waning and partial melting of the Pleistocene ice sheets, up to the present, is $.18 - .05 = .13$ km. For the total rise of sea level in this time we must of course add the effect due to the delivery to the ocean of the products of ordinary stream erosion.

OBITUARY

LEE CLEVELAND CORBETT

THE death of Dr. Lee Cleveland Corbett, which occurred on July 13, 1940, at his home in Takoma Park, D. C., came as a surprise to most of his friends. Though it was known following his return on the first of April from Florida that he was not well, few realized that his condition was critical.

The principal steps of Dr. Corbett's professional career after graduating from Cornell University, College of Agriculture, in 1890, constitute a continuous progression. From 1891–3 he was assistant horticulturist at the Cornell University Experiment Station under Dr. L. H. Bailey. In 1893 he went to South Dakota in the pioneer period of agricultural education there, as professor of horticulture and forestry in the State Agricultural College and Experiment Station, where he remained two years, going in 1895 to the University of West Virginia as horticulturist, where he continued until April, 1901, when he was appointed as horticulturist in the U. S. Department of Agriculture.

By a then recent Act of Congress, the tract of land across the Potomac River from Washington to become known later as Arlington Farm, had been turned over to the Department of Agriculture. Dr. Corbett's chief assignment, and his principal activity for several years, was the development of this land for experimental

[6] *Op. cit.*, p. 9.

purposes and the construction of the necessary buildings.

It was on July 1, 1901, that the Bureau of Plant Industry came into official being and under it there were combined into a single major administrative organization the several independent plant divisions, the heads of which had previously reported directly to the secretary. Shortly after Dr. Corbett came to the department, the Office of Horticulture was organized under his direction as one of the coordinate units in the Bureau of Plant Industry and in which the vegetable work of the Department was placed. Later, investigations in floriculture and landscape gardening were added. As the pioneer work at Arlington Farm was gradually accomplished, thus requiring less of Dr. Corbett's attention, the vegetable and floricultural work was given an increasingly large proportion of his time. In 1913, he was appointed assistant chief of the Bureau of Plant Industry. At the same time the Office of Horticulture and the Office of Pomology were combined into a single administrative unit in charge of the late A. V. Stubenrauch. When the latter, in July, 1914, resigned to return as professor of pomology to the University of California with which he had previously been connected, Dr. Corbett rather gladly transferred back to division work, succeeding Mr. Stubenrauch as the administrative head of the Office of Horticulture and Pomology.

In the fall of 1920, he made a trip to England and the Continent, particularly France and Holland, visiting the nursery growing centers of these countries in the interests of the root-stock investigations which were being inaugurated here.

Dr. Corbett served as head of the Office of Horticulture and Pomology until November, 1928, when he welcomed a transfer from administrative work to a field of research in which he had long been greatly interested, namely, a study of the causes leading to the development of horticultural industries in particular areas and their subsequent history, including both advancement and decline in different instances. This involved the appraisal of many factors as the suitability of the varieties grown, time of ripening, competing crops, regional costs of production, distribution facilities and other factors which might influence trends in the industries under investigation. The most of his time until his retirement, January 31, 1938, was devoted to this line of research, but during the last year of his service, he made a three months' plant exploration trip to South America on a special assignment in the Bureau of Plant Industry.

In the earlier years of his professional work he was a prolific writer, being the author of a considerable number of bulletins from the South Dakota and West Virginia Experiment Stations, and of a much larger number relating principally to vegetable growing and ornamental horticulture after he came to the Department of Agriculture. He also contributed to several yearbooks of the department and to numerous publications outside the department; he was the author of two books—"Garden Farming," a rather comprehensive work on vegetable culture, and "Intensive Farming."

Dr. Corbett was born at Watkins, N. Y., on October 21, 1867. In addition to the degree of B.S. conferred on graduation from Cornell University, he received the degree of M.S. in 1896 from the same institution, and the honorary degree of doctor of agriculture in 1921 from the University of Maryland. He held membership in numerous scientific and other organizations, including the American Association for the Advancement of Science of which he was a fellow, Vegetable Growers' Association of America, American Society for Horticultural Science of which he was president in 1914, National Potato Association of America, National Geographic Society, Washington Academy of Sciences, Botanical Society of Washington, Cosmos Club, and others.

Surviving Dr. Corbett are his widow, Mrs. Louise Phillips Corbett, and five children by his deceased wife, Mrs. Evelyn Northrup Corbett—Ruth, Frances (Mrs. Colston E. Warne, Amherst, Mass.); Dr. Roger B., director, Extension Service, University of Connecticut, Storrs; Laurence W., with Northrup, King and Co., seedsmen, Minneapolis, Minn.; Lee Thurston, lawyer, Rochester, N. Y., and a number of grandchildren.

H. P. GOULD

U. S. HORTICULTURAL STATION,
BELTSVILLE, MD.

RECENT DEATHS

THE body of Dr. John Eliot Wolff, professor emeritus of geology at Harvard University, was found after a twenty-seven-hour search on August 12 in the Mojave Desert, where he had gone on a one-day camping trip. He was eighty-two years old.

DR. FREDERICK EHRENFELD, head of the department of geology and mineralogy at the University of Pennsylvania, a member of the faculty for forty-three years, died on August 16. He was sixty-eight years old.

DR. MILICENT WASHBURN SHINN, specialist in child psychology, editor of the *Overland Monthly* from 1883 to 1894, died on August 14 at the age of eighty-two years.

FRANKLIN HENRY HOOPER, editor-in-chief of the Encyclopaedia Britannica from 1932 until 1938, who was associated with the publication for thirty-nine years, died on August 14 from injuries received when struck by a truck. He was seventy-eight years old.

ARTHUR H. FLEMING, lumber manufacturer of Los Angeles, died on August 11 at the age of eighty-four years. He gave a sum amounting to more than five million dollars to found the California Institute of Technology at Pasadena. Dr. R. A. Millikan, chairman of the executive committee of the institute, made the following tribute: "It takes great courage and boldness to start any new enterprise, and it was Mr. Fleming's energy and boldness that made it possible to start California Institute on its present type of career."

SIR ALFRED BOURNE, formerly professor of biology in the Presidency College, Madras, died on July 14 at the age of eighty years.

DR. WILLIAM RUSSELL, emeritus professor of clinical medicine at the University of Edinburgh, past president of the Royal College of Physicians, died on August 11. He was eighty-eight years old.

SCIENTIFIC EVENTS

THE BEIT MEMORIAL FELLOWSHIPS

AT a meeting of the trustees of the Beit Memorial Fellowships for Medical Research, which was held on July 17, Dr. A. J. Clark, professor of materia medica in the University of Edinburgh, was appointed a member of the Advisory Board in the place of Professor W. Bulloch, who resigned after twenty-nine years' service. Dr. Paul Fildes was appointed honorary secretary on the resignation of Professor T. R. Elliott, who had served for ten years. Out of the twenty-seven present fellows nine have at their own request given up their fellowships for more direct service during the war.

The following elections were made, all with permismission for each fellow to be relieved at any time for war work.

SENIOR FELLOWSHIP, (£700 a year)

T. A. H. Munro, M.B., Ch.B., F.R.C.P. (Edinburgh). To continue his studies of inheritance in mental disorders. At the Royal Eastern Counties Institution. Colchester.

FOURTH YEAR FELLOWSHIPS, (£500 a year)

J. G. Chalmers, B.Sc. (Glasgow). A.I.C. To continue his chemical studies of polycyclic hydrocarbons in experimental cancer formation. At the research department of the Glasgow Royal Cancer Hospital.

T. R. R. Mann, M.D. (Lwow, Poland), Ph.D. (Cambridge). To extend his work on intracellular metalloprotein compounds, especially of red blood cells. At the Molteno Institute of Biology, University of Cambridge.

JUNIOR FELLOWSHIPS, (£400 a year)

M. Abercrombie, B.A. (Oxford). Junior research fellow, Queen's College, Oxford. To study amputation neuromata and the degeneration and regeneration of mammalian nerves. At the department of zoology, University of Oxford.

D. A. K. Black, M.D. (Gold Medal) (St. Andrews) M.R.C.P. (London). Research student of Medical Research Council. To study metabolic and bone marrow changes in the repair of severe anemias. At the Nuffield department of medicine, Oxford.

G. C. Butler, Ph.D. (Toronto). 1851 Exhibition Science Research Scholar. To study the purification and properties of gonadotropic hormone from the pituitary gland. At the department of pathological chemistry, University College Hospital Medical School, London.

J. L. Madinaveitia, D.F. (Madrid), Ph.D. (Edinburgh). Honorary research fellow in chemistry, Manchester University. To study diffusing factors in bacterial filtrates and snake venoms. At the department of chemistry, University of Manchester.

S. M. Partridge, B.Sc., Ph.D. (London). Lister Institute student in biochemistry. To study the chemical nature of the antigen of Shiga dysentery bacteria. At the Lister Institute of Preventive Medicine, University of London.

D. Whitteridge, B.A., B.M., B. Ch. (Oxford). Demonstrator in physiology, Oxford. To study electrical records from brain after concussion or severe injury. At the Nuffield department of surgery and the department of physiology, University of Oxford.

P. C. Williams, B. Sc. (London). Research grantee of Medical Research Council. To study pituitary hormones and their excretion in clinical pathological conditions. At the Courtauld Institute of Biochemistry, Middlesex Hospital, London.

THE TOTAL SOLAR ECLIPSE OF OCTOBER 1

An expedition will be sent to observe the total solar eclipse in Brazil on October 1, under the joint auspices of the National Geographic Society and the National Bureau of Standards, to be led by Dr. Irvine C. Gardner, chief of the Optical Instruments Section of the bureau. Other members of the party are: Dr. E. O. Hulburt, of the Naval Research Laboratory; Dr. Paul A. McNally, S.J., director of the Observatory of Georgetown College; Dr. Carl C. Kiess, spectroscopist, and Dr. Theodore R. Gilliland, radio research specialist, the National Bureau of Standards, and Richard H. Stewart, staff photographer of the *National Geographic Magazine*.

The expedition will sail on August 24 for Recife, Pernambuco. After reaching Recife it will journey overland by truck and automobile through the city of Campina Grande to the neighborhood of the village of Patos, nearly 200 miles from the coast. Patos lies five miles south of the center line of the total eclipse; the instruments will be set up directly on that line. The

observation post will be in a plateau country in the heart of one of the most important cotton-growing regions. The presence at Patos of a cotton gin and staff men of an American cotton company will make it possible for the expedition to find facilities which otherwise would be lacking.

The announcement issued by the National Geographic Society points out:

> The total eclipse of the sun of October 1 offers unusual opportunities for satisfactory observation because it traverses a dry region in which weather conditions are likely to be favorable. The sun will be unusually high in the heavens (approximately 54°) at the time of totality, and the period of darkness will last for nearly five minutes. To take advantage of these conditions, the National Geographic-Bureau of Standards expedition has built two special spectrographs, each capable of photographing a portion of the sun's spectrum 40 inches long. In addition the expedition designed and built especially for the October 1 eclipse, two small, compact telescope-type corona cameras. These will be used to photograph the corona, the delicate halo that extends outward around the sun but which can be seen only during total eclipses. Included in the equipment also will be the large telescope camera designed by Dr. Gardner several years ago with which he has photographed solar eclipses on National Geographic Society expeditions in Russia, and on Canton Island in mid-Pacific.

After the eclipse the expedition will move to Campina Grande, a city of approximately 90,000 inhabitants, to carry on the necessary laboratory work in the development and preparation of the photographic films. The party will sail from Recife on its return voyage to the United States on October 14, and expects to reach New York about October 23.

THE DEDICATION OF THE NEW OYSTER LABORATORY AT MILFORD, CONNECTICUT

THE dedication on August 1 of the new oyster laboratory at Milford, Conn., of the Fish and Wildlife Service, as reported in *The Fisheries Bulletin*, marked a significant attainment in the twenty-three years of investigation of oyster problems in the general area of Long Island Sound. Oyster investigations at Milford were initiated in 1917, when the Connecticut Oyster Farms Company provided one room in their office building which was outfitted as a laboratory and used only during the summer months. Although the earlier investigations contributed a great deal toward the solution of various oyster-cultural problems, the program was handicapped by the necessity of closing the laboratory from September to May of each year.

In 1932, at the invitation of the Connecticut Shellfisheries Commission, the laboratory was moved to a small shack on the state dock and full-time investigations were initiated. At that time arrangements were completed with Yale University for laboratory space in which to conduct experiments of a more elaborate and technical nature. In 1937 the Connecticut State Legislature passed a bill that deeded a portion of the land at Milford to the former Bureau of Fisheries. Construction of a new permanent laboratory was made possible through the allocation of P. W. A. funds by Secretary of the Interior Ickes, in his capacity as Public Works Administrator.

The Milford Station is the center from which oystermen obtain data regarding the condition of oysters on various beds, the expected time of setting and the presence of starfish and other enemies, as well as information which may be useful to the oyster farmer. Through the facilities of the new laboratory scientific knowledge necessary for the conservation and management of the oyster resources will be more readily obtainable.

The dedication of the Milford Laboratory was held in conjunction with the joint annual convention of the Oyster Growers and Dealers Association of North America, the National Shellfisheries Association and the Oyster Institute of North America. Sessions were held at New Haven, Conn., on July 31 and August 2, and at Milford on August 1. W. C. Henderson, assistant director of the Fish and Wildlife Service, delivered a message from Secretary Ickes. Drs. Paul S. Galtsoff, H. F. Prytherch, Victor L. Loosanoff and Messrs. James B. Engle and R. O. Smith of the shellfisheries investigations staff of the Division of Scientific Inquiry, and J. F. Puncochar, of the technological staff of the Division of Fishery Industries, presented reports on their investigations at the various sessions.

SYMPOSIUM ON HYDROBIOLOGY AT THE UNIVERSITY OF WISCONSIN

A SYMPOSIUM on hydrobiology will be held at the University of Wisconsin on September 5, 6 and 7, funds for which have been provided by the Wisconsin Alumni Research Foundation.

Forty-two scientific papers discussing the history, geology, physics, chemistry, bacteriology, botany and zoology of bodies of water in all parts of the world are listed in the program. Both social and economic aspects of inland lakes and streams will be given considerable attention at the symposium. The conservation of water in lakes and streams and how to use these bodies of water to the best advantage for fish culture and recreation will be discussed. Several papers on Wisconsin lake studies will be given, since research work on the state's lakes and streams has been carried on for a number of years by the Wisconsin Geological and Natural History survey.

Among the fifty-five investigators expected to be

present there are two scientific men from abroad: Kaare M. Ström, Norwegian limnologist from the Geological Museum at Oslo, who will present a paper on "The Sediments of Norwegian Lakes," and Dr. S. F. Snieszko, formerly professor of bacteriology at the University of Krakow, Poland, who will read a paper on "Pond Fish Farming in Poland." Dr. Snieszko is now at the University of Maine.

A dinner in honor of the eighty-ninth birthday of Dr. E. A. Birge, president emeritus of the University of Wisconsin, at which Dr. C. A. Dykstra, president of the university, will preside, will be given on September 5.

THE OPTICAL SOCIETY OF AMERICA

THE twenty-fifth annual meeting of the Optical Society of America will be held in Rochester, N. Y., with headquarters at the Hotel Sagamore, on October 3, 4 and 5. It is pointed out in the official announcement that the selection of Rochester for the twenty-fifth annual meeting is a natural one, in view of the importance of the city in the optical industry and of the part which Rochester members have played in the organization and development of the society. Special arrangements are being made to commemorate the twenty-fifth annual meeting, and a local committee, appointed by the Rochester Section, is active in plans to make the meeting successful.

The program as tentatively arranged is as follows:

THURSDAY, OCTOBER 3

Morning—Contributed papers.

Afternoon—Reserved for visits to industrial plants in Rochester.

Evening—Lecture by Edwin H. Land illustrated by experiments and by exhibits of Polaroid and some of its applications in the field of photography.

FRIDAY, OCTOBER 4

Morning—Symposium of invited papers. These are intended to comprise a review of the development of optics with emphasis on recent significant advances in the field, thus endeavoring to present both the historical and contemporary aspects.

Afternoon—Reserved for visits to industrial plants in Rochester.

Evening—Complimentary dinner at the Oak Hill Country Club, at which members and their wives will be the guests of the Bausch and Lomb Optical Company. The dinner, at which informal dress will be in order, will be featured by a recognition of the active Charter Members of the Society and the award of the first Adolph Lomb Medal.

SATURDAY, OCTOBER 5

Morning—Contributed papers.

Afternoon—Reserved for additional contributed papers as required.

Members desiring to read papers at the meeting are requested to send abstracts to Arthur C. Hardy, Secretary, Optical Society of America, Massachusetts Institute of Technology, Cambridge, Massachusetts. They should be in his hands not later than September 11. The chairman of the program committee is A. W. Kenney, of the E. I. du Pont de Nemours and Company. Mr. Hardy has been filling, until the next annual meeting, the vacancy created by the resignation as secretary of L. B. Tuckerman.

SCIENTIFIC NOTES AND NEWS

DR. THOMAS BARBOUR, director of the Museum of Comparative Zoology, Harvard University, has been elected a foreign member of the Linnean Society of London.

THE retirement is announced of Dr. Charles Albert Browne, supervisor of chemical research in the Bureau of Chemistry and Soils of the U. S. Department of Agriculture. Dr. Browne has been connected with the Department of Agriculture since 1906.

G. G. BROWN, professor of chemical engineering at the University of Michigan, was recently the recipient of the Hanlon Award of the Natural Gasoline Association of America for "outstanding contributions in the field of natural gasoline."

THE title of dean emeritus has been conferred on Dr. Langley Porter, of the Medical School of the University of California. He had been associated with the school since 1918.

COLONEL HAROLD W. JONES, librarian of the Army Medical Library, Washington, D. C., was reelected president of the Medical Library Association at its annual meeting in Portland, Ore., which was held on June 25, 26 and 27. Dr. Robert E. Schlueter, St. Louis, was elected vice-president and Miss Anna C. Holt, Harvard Medical School Library, Boston, was reelected secretary. The convention in 1941 will be held at the Medical School of the University of Michigan.

THE following officers for the year 1940–41 were elected at the annual meeting of the Royal Society of Queensland: *President,* Dr. F. W. Whitehouse; *Vice-presidents,* H. A. Longman and Professor H. R. Seddon.

DR. C. E. MCCLUNG, who recently retired as director of the biological laboratories at the University

of Pennsylvania, has been appointed visiting professor of biology at the University of Illinois.

DR. HENRY J. MASSON, professor of chemical engineering and director of the Graduate Division of the College of Engineering of New York University, a member of the faculty for twenty-three years, has been appointed acting director of the evening division of the College of Engineering. He succeeds Professor Sampson K. Barrett, who will devote his full time to teaching.

DR. WILLARD L. VALENTINE, associate professor of psychology at the Ohio State University, has been appointed chairman of the department of psychology at Northwestern University.

DR. ROGERS D. RUSK, since 1928 associate professor of physics at Mount Holyoke College, has been appointed chairman of the department to succeed Dr. Elizabeth R. Laird, who retired in June.

DR. BRADLEY T. SCHEER, research assistant in the division of biochemistry and physiology at the Scripps Institution of Oceanography of the University of California at La Jolla, has been appointed instructor in zoology and physiology at West Virginia University.

DR. T. G. H. JONES, lecturer in chemistry at the University of Queensland, has been appointed professor of chemistry in succession to the late Professor L. S. Bagster.

DR. JASPER L. STUCKEY, who resigned as state geologist of North Carolina in 1926 to become professor of geology in the State College at Raleigh, has returned to the survey to succeed H. J. Bryson, who resigned in the spring to take a position with a mining concern. Dr. Stuckey, who is head of the department of geology at the State College, will continue as a member of the faculty, devoting himself on a part-time basis to the administrative functions of state geologist. T. G. Murdock, for a number of years a mining engineer in Chile, Peru, and more recently in Cuba, who has been appointed assistant state geologist, took up his work on August 1.

DR. ROBERT JACKSON NOBLE, chief biologist of the Department of Agriculture of New South Wales, has been appointed under secretary for agriculture in succession to G. D. Ross, who retired recently because of ill health.

DR. W. J. GERTSCH, associate curator of spiders of the American Museum of Natural History, has returned from an expedition to Utah and Arizona bringing with him a collection of 5,000 spiders, including black widow spiders and tarantulas. Many of the specimens brought back were found at altitudes of 8,000 to 10,000 feet.

DR. MARY SWARTZ ROSE, professor of nutrition at Teachers College, Columbia University, has leave of absence until July 1, 1941, when she will retire.

DR. FRANCIS SCOTT SMYTH, of the department of pediatrics of the University of California, will spend six months at the Medical School of the University of Buenos Aires working in the department of physiology with Dr. B. A. Houssay, professor of physiology.

DR. HERNÁN AGUILAR, associate professor of medicine at Buenos Aires, has sailed for the United States to work on thoracic surgery in the Medical School of Washington University, St. Louis.

DR. SHARAT K. ROY, curator of geology of the Field Museum, Chicago, who is conducting an expedition to various localities in Wyoming, Colorado and South Dakota and also to various eastern states to collect specimens relating to structural and dynamic geology, left Chicago on August 16. He expects to be absent for about ten weeks.

LEÒN GRODSINSKY, of the Division of Phytopathology of the Ministry of Agriculture, Buenos Aires, sailed on August 9 for home. He had spent a year in phytopathological and mycological study in the United States. He was a delegate to the eighth American Scientific Congress held in Washington, D. C., from May 10 to 21.

OWING to the war, the meeting of the Australian and New Zealand Association for the Advancement of Science has been postponed indefinitely.

THE Second National Medical Congress in Peru is to be held this month in Arequipa under the auspices of the Peruvian Government in connection with the four hundredth anniversary of the founding of the city. There will be sections for medicine, surgery, hygiene and social medicine.

THE Allegheny Section of the Society for the Promotion of Engineering Education will hold its sixth annual meeting at the Carnegie Institute of Technology, Pittsburgh, on October 25 and 26. The subject of the meeting will be "The Coordination of Engineering Curricula." The address of welcome will be made by Dr. Robert E. Doherty, president of the institute. The Engineers' Council for Professional Development will meet in Pittsburgh on October 24.

A CONFERENCE on Soil Mechanics and Its Applications will be held at Purdue University, West Lafayette, Ind., from September 2 to 6, under the auspices of the Committee on Foundations and Soil Mechanics of the Civil Engineering Division of the Society for the Promotion of Engineering Education and the School of Civil Engineering and department of engineering extension of Purdue University.

THE *Journal* of the American Medical Association states that Dr. David Riesman, professor of the history of medicine, who died on June 3, bequeathed his brain to the Wistar Institute of Anatomy for research purposes. He made a bequest of $1,000 to the College of Physicians of Philadelphia; of $500 to the Philadelphia County Medical Society and of $2,000 to the medical board of the Philadelphia General Hospital "to encourage research on the part of the staff."

A NEW bird exhibit, known as the Snow Mountains-New Guinea Group, in the Whitney Memorial Hall of the American Museum of Natural History was dedicated by Mrs. Harry Payne Whitney on August 20. The group, which is a replica of bird life 11,000 feet up in the little-known Snow Mountains of Netherland New Guinea, was presented to the Museum by Richard Archbold, and was collected by Dr. A. L. Rand, of the American Museum staff, during the 1938–39 Archbold-New Guinea Expedition of the museum. The principal speaker at the ceremony was the Honorable J. S. Schurman, Consul General of the Netherlands.

ACCORDING to the London *Times* the department of zoology of the British Museum has received from Admiral Sir Sidney Bailey a series of mounted heads of North American ungulates, including a very fine moose and some exceptionally good wapiti and caribou. The collection also contains a head of a Rocky Mountain goat. Another gift to the same department is a series of 10 skulls with horns of African antelopes from the Dinder River District of the Sudan, presented by Mr. T. W. H. Dore. This collection includes some skulls of Soemmerring's gazelle and of the Sudan race of the roan antelope. The department of geology has acquired from the committee of the Royal Literary and Scientific Institution, Bath, a collection of 99 invertebrate fossils from the mesozoic of Australia, including a number of type and figured specimens.

A WESTINGHOUSE Electric and Manufacturing Company exhibit at the Golden Gate Exposition, San Francisco, is a model of the giant 200-inch Mount Palomar telescope by means of a one-tenth size operating scale model constructed at the cost of $25,000. The model is owned by the California Institute of Technology, which has used it for more than a year for actual astronomical observations. Although it was built primarily to illustrate the construction of the Palomar telescope, the 20-inch mirror makes it valuable for practical use. Throughout the construction, technicians of the company collaborated with experts from the California Institute and the Mount Wilson Observatory. Reproduced in the model is the horseshoe bearing, which in the big telescope makes it possible to swing the shaft of the instrument with 1-165,000th of a horsepower. The original bearing itself weighs 400,000 pounds and is 46 feet in diameter.

ACCORDING to *The Collecting Net,* a new instrument, the Continuous Plankton Recorder, has been received by the Oceanographic Institution at Woods Hole, which will have the recorder on loan from Professor A. C. Hardy, of University College, Hull, England, for the duration of the war. The instrument will be used to record the density of living matter in the ocean, and has the advantage over other forms of collecting apparatus in that it does not have to be periodically removed from the water. It can be towed by a ship and will record the fluctuation in density of living matter along the course. Shaped like a torpedo, the recorder contains a spool of gauze which unwinds as the plankton is caught at the rate of about an inch for every mile that the ship travels. About twenty of these recorders are now in existence, and the two at the Oceanographic Institution, which will be used by the *Atlantis,* are the only ones outside of England.

THE London *Times* reports that two films concerned with problems of British agriculture recently were shown privately in London to a specially invited audience which included several members of Parliament. The films were presented by John Grierson, who spoke of the contribution that films could make towards bridging the gap in understanding between townsmen and countrymen, and in making known to the farmers the important work that agricultural research stations are doing. "Speed the Plough" and "Protection of Fruit" were, Mr. Grierson said, the first of many films which might be made; production had just begun on a film describing the grass breeding experiments being carried out by Professor Sir Reginald George Stapledon at the University of Aberystwyth. "Speed the Plough," which was made under the supervision of Professor Scott Watson, is designed to show the new mechanical and scientific methods that are being adopted by farmers. The film on the "Protection of Fruit" was made in collaboration with agricultural research stations in Great Britain and in Holland and Palestine. It has been specifically made for showing to audiences of farmers.

THE *Journal* of the American Medical Association states that the Kaiser Wilhelm Society for Scientific Research has been increased by the addition of a new institute of biophysics, constructed in Frankfort on the Main with the support of the city and the cooperation of the university. The building contains twenty research laboratories. There is here the first laboratory for the diagnosis of radium-intoxicated persons, that is, occupational diseases like those of miners and employees of radium factories. The principal tasks of the institute are the investigation of the biophysical problems of the effects of roentgen and radium rays

on the tissues, the problems of the medical and the technical use of light, the effects of electric waves and of radioactive substances and the study of idioradiation of living organisms, especially of the hypothetical rays of cell division. The new institute will be under the direction of Professor Rajewski.

DISCUSSION

A STANDARDIZATION OF OSMOTIC PRESSURE AS A TERM

THE vast amount of confusion which arises from certain physiological terms makes it advisable to bring about a standardization of such terms so that each term will convey a definite and clear concept. Osmotic pressure is a term which most of all needs standardization. At national botanical meetings in discussions involving osmotic relations it is apparent that no two of the botanists have quite the same idea or definition for osmotic pressure. Authors of botanical texts also show a diversity of opinion on the subject. Apparently, some authors either are undecided about a definition or regard the usual definition as unsatisfactory because they present no definition for osmotic pressure.

The definition of osmotic pressure which is generally accepted is that it is or is equal to the maximum hydrostatic pressure exerted by a solution when it is in equilibrium with pure water at the same temperature. This definition is poor and confusing because it either infers erroneously that osmotic pressure is the result and not the cause of osmosis or merely evaluates the pressure in terms of another variable without explaining or describing the nature and the cause of osmotic pressure. Shull[1] in explaining an example which he cited states that "the actual osmotic pressure here is the pressure of the water molecules passing through the bladder membrane." This idea of Shull is a splendid one and is the one which should be universally adopted, because it describes osmotic pressure as a dynamic and functional force, and not as an end result, which it certainly is not.

Since the author has given this subject considerable thought and has discussed it at great length with Dr. Lyons, of Dartmouth University, and has discussed it somewhat with several other plant physiologists, he wishes to propose the following standardization for osmotic pressure. Please bear in mind that the proposed standardization is merely an expansion of Dr. Shull's sound and logical idea.

Water molecules diffusing through a membrane exert a diffusion pressure. Since this special type of diffusion is called osmosis, the diffusion pressure of the water molecules tending to diffuse through the membrane can logically be referred to as *osmotic pressure*. It should be understood that osmotic pressure is not the result of osmosis but that it is the cause of osmosis, and that osmotic pressure is both the diffusion pressure exerted inwardly by water diffusing into the cell and the diffusion pressure exerted outwardly by water diffusing out of the cell.

The osmotic pressure of the water in the vacuole of a plant cell is increased by turgor pressure and decreased by the solute concentration of the cell sap. The water in the plant vacuoles at a given turgor pressure has a maximum osmotic pressure when there are no solutes present. With increased concentrations of solutes in the cell sap, the osmotic pressure of its water is proportionally reduced. To say that the osmotic pressure of the cell sap of a given cell is 1.5 M means that the diffusion pressure of the water in the cell sap is the same as the diffusion pressure of the water in a 1.5 M aqueous solution of cane sugar. To say that the osmotic pressure of the cell sap of a given cell is 4.2 atmospheres means that the diffusion pressure of the water in the cell sap is 4.2 atmospheres less than that of pure water at the same temperature and external pressure. Each gram molecular weight of cane sugar or of any non-electrolyte dissolved in enough water to produce a total of one liter of solution reduces the diffusion pressure of the water molecules 22.4 atmospheres at standard conditions (0° C. and 760 mm. barometric pressure).

The rate of osmosis into a cell is directly proportional to the difference between the osmotic pressure of the water surrounding the cell and the osmotic pressure of the water inside the cell.

Water diffuses osmotically from the cell with the greater osmotic pressure to the adjacent cell with the less osmotic pressure. For example water diffuses from the cell with an osmotic pressure of .2 M to a cell with an osmotic pressure of .4 M. Distilled water which has an osmotic pressure of *zero molar* will diffuse into a cell with an osmotic pressure of .2 M. It must be remembered that the greater the solute concentration the less is the osmotic pressure of the solvent (water), because solute molecules decrease the activity of the solvent molecules.

If one defines osmotic pressure as the diffusion pressure of solvent (water) molecules tending to diffuse through a membrane, there is no need for the terms "diffusion pressure deficit" and "suction tension." These latter terms are not desirable. The term "diffusion pressure deficit" erroneously conveys the idea to many that a cell with a diffusion pressure deficit is in an abnormal state. The term "suction tension" indicates that a cell takes up water by a sucking action which in reality does not exist.

The above standardization for osmotic pressure as a term is advisable, therefore, (1) because it is simple, sound and logical, (2) because it does not necessitate the use of the undesirable terms "diffusion pressure

[1] Charles A. Shull, "A Textbook of Botany Revised." Vol. II. "Physiology," p. 16. Cincinnati: American Book Company, 1930. (Coulter, Barnes, and Cowles.)

deficit" and "suction tension," and (3) because there is no need for the term "net osmotic pressure."

H. C. EYSTER

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STRAIN SUSCEPTIBILITY TO THE EUROPEAN CORN-BORER AND THE CORN-LEAF APHID IN MAIZE

THE European corn-borer (*Pyrausta nubilalis* Hubn.), a chewing insect, and the corn-leaf aphid (*Aphis maidis* Fitch), a sucking insect, are both serious pests of *Zea mays* L. Differential susceptibility to the corn-borer among corn strains has been recognized for several years. The authors have recently found: (1) a differential susceptibility among corn strains to the aphid and (2) significant correlations in the degree of strain susceptibility to these two widely different pests.

Among corn hybrids grown in northwestern Ohio, significant correlation coefficients of 0.570 and 0.844 were found in different seasons between expected aphid infestation based on aphid susceptibility ratings of the parent inbred lines and estimates of actual corn-borer infestations. Estimates of aphid abundance were based on actual counts of infested plants. Estimates of corn-borer abundance were based both on counts of infested plants and on stalk breakage associated with corn-borer damage. These correlation values are as high as have usually been found between corn-borer infestation counts on the same corn strains in different seasons. Correlations between corn-borer and aphid infestations on the same plots may, however, be very low because of competition between the insect species.

The relation has one immediate practical application. For corn-growing areas where the corn-borer is a serious pest, the differential strain susceptibility to corn-borer infestation provides a basis of major importance in classifying breeding material. The chief difficulty in measuring strain susceptibily has been burdensome techniques. In general, the choice lay between (1) adequate sampling of stalks with either natural or manual infestation and (2) feeding etiolated leaves of different strains to young larvae in the laboratory. Manual infestation reduces the number of plants needed for dissection but adds the requirement of rearing moths and applying eggs; it adds also an uncertainty of simulating natural conditions. Further, stalk dissection readings are subject to a large error unless properly timed with reference to the usually different maturity of the strains under study. Laboratory feeding also requires the rearing of moths and handling of eggs and larvae in addition to providing a constantly fresh supply of food.

Counts of aphid colonies, on the other hand, can be made rapidly. The evidence indicates that the degree of aphid infestation may be used as an index to the degree of strain susceptibility to the corn-borer, at least for preliminary classifications of corn-breeding material.

L. L. HUBER

G. H. STRINGFIELD[1]

OHIO AGRICULTURAL EXPERIMENT STATION, WOOSTER

AN EXPERIENCE OF GENERAL ANESTHESIA

HAVING been occupied for some time with a disquisition on "Biology and Substance" I have had an experience in the last two days that furnished me facts and reflections on the subject that interests me a good deal and it has occurred to me might be of some interest to other people.

The discovery had recently been made by myself and my dentist that I had two teeth that were worse than useless, with the consequent decision that they had better be extracted. Passing over details in preparation for this I merely mention that the decision was that there should be general instead of local anesthesia —heretofore never experienced by me. Early in the morning I found the anesthetist with some sort of a contrivance beside my head with the instruction that I breathe perfectly naturally. My only response so far as I remember was the foolish question as to whether this was "N O" or "N O 2," to which she replied "N O."

Although I was naturally not holding the stop watch, as near as I could judge it could not have been many minutes at most before I was completely gone so far as my consciousness was concerned. When I came to my senses forty-five minutes later, as I was told, I was aware of some soreness in my mouth and not much later I was wide awake and restored to my regular bed in the hospital. What particularly interests me is the problem of what, during the period of nonexistence so far as my consciousness was concerned, that substance, "N O," was doing to me.

I note, first of all perhaps, that from my little knowledge of chemistry and biochemistry I know that nitrogen and oxygen were playing important parts in my existence up to the time my existence disappeared. Their action on me must, however, have been very different when the two were combined to make "N O" than when either was thus uncombined. How is such a thing possible? How was it that the combination of those two chemical elements could produce such a striking effect as compared with what either of them could do alone or in other combinations; and the gist of my inquiry is as to the latent qualities or properties which each separate chemical substance

[1] The Departments of Entomology and Agronomy, Ohio Agricultural Experiment Station, and the Division of Cereal Crops and Diseases, Bureau of Plant Industry, U. S. Department of Agriculture, cooperating.

must have that can be actualized in no other way than by their coming together to make the compound substance put me out of conscious existence. No amount of chemical examination in the laboratory, so far as I know, would indicate that the two would unite and make a substance that would have such a remarkable effect.

I raise the question of whether this striking example of chemical transformation is more or less typical of all chemical transformation. Is there anything about the phenomenon that we can say intelligently, other than that chemical transformation in general is a process of bringing into actuality the potential qualities that the elements have, which can be discovered in no other way than just that of their acting on one another; and that when they have made a compound substance, the action of that substance can be discovered in no other way than by trying it on a living being more or less like myself?

I wonder if this sort of thing which is now common knowledge to us moderns, does not throw some light on the seemingly endless speculation of the philosophers and theologians, not to say some expert scientists, with reference to what the idea of substance really amounts to.

It looks to me as though the idea of potentiality, of which Aristotle made much as to that particular meaning of the Greek *dunamis,* has been overly neglected in the natural knowledge of the modern era.

WM. E. RITTER

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THE CONQUEST OF LEPROSY

IN 1927 my wife and I visited Dr. Douglas Collier and his wife, Dr. Mary Collier, at their mission station at Nan, Siam. We also went to Chiengmai, where Dr. J. W. McKean was in charge of the admirable institution for the care of lepers. These medical missionaries worked under the auspices of the Presbyterian Board, and we felt that their work was of the utmost importance to the country, and a credit to their American supporters.

The years have gone by, Dr. McKean has retired, and now lives in California; Dr. Collier has moved to Chiengmai, and taken up the work with the lepers. All this might not call for special comment were it not that at last after long years of investigation, it appears probable that leprosy may be actually conquered. The experimental work at Chiengmai, in its present form, was initiated by Dr. M. J. Oberdoerffer, a young German who had worked in Africa under the British Empire Leprosy Relief Association. It appeared that the eating of colocasia predisposed to the development of leprosy, apparently due to a toxic substance also found in partly decomposed fish. Using this method, it was found possible to cause monkeys to acquire leprosy, something which it had seemed impossible to do before. The conclusion was reached that injury to the adrenal glands was the more immediate cause of trouble, and short-wave radiation was used to stimulate these glands, with satisfactory results. However, it was suggested that possibly the use of diphtheria antitoxin might be beneficial, as in diphtheria a toxin is liberated which damages the adrenal glands. In diphtheria, antibodies are formed, but this seems not to be the case in leprosy. When the work had reached this promising stage, Dr. Oberdoerffer, on account of poor health, felt obliged to leave Siam. Dr. Collier continued the work, now using toxoid instead of the old antitoxin. The results have been extraordinary, and Dr. Collier states:

> In the use of toxoid and antitoxin we have a treatment which far exceeds any method yet known. Results are obtained in a few weeks, which formerly were seen only after months or years of treatment. While the early cases are less spectacular in response, all types seem to be benefited.

It is reasonably hoped to immunize the children and associates of lepers, and so in time eradicate the disease. All these matters are set forth in a paper just received, published in the *Journal of the Thailand Research Society* (Bangkok), February, 1940.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO

SCIENTIFIC BOOKS

THE KOSHER CODE

The Kosher Code of the Orthodox Jew. Being a literal translation of that portion of the sixteenth-century codification of the Babylonian Talmud which describes such deficiencies as render animals unfit for food (Hilkot Terefot, Shulhan 'Aruk); to which is appended a discussion of Talmudic anatomy in the light of the science of its day and of the present time. By S. I. LEVIN, senior rabbi of Minneapolis, and EDWARD A. BOYDEN, professor of anatomy, University of Minnesota. xx + 243 pp. Minneapolis: University of Minnesota Press. 1940. $4.50.

THE anatomist and embryologist must visit, if not frequent, the slaughter-house. The late Professor Minot remarked, in his quiet way, "It is hell," as he sent the reviewer there. Dr. Boyden's errand, fifteen years ago, was to find how often partly divided and supernumerary gall bladders occur in calves and sheep

—2,500 of each. In the abattoir "kosher cutters" from the local synagogues were on hand, inspecting viscera and mysteriously accepting as kosher or kasher the carcasses ritually fit, but occasionally rejecting one as terefah or terephah ("torn of beasts"). They told Dr. Boyden that such anomalies as he sought "were already described in the Shulhan 'Aruk"—the code of Jewish law under which they served. This code consists of text written by a sixteenth-century Palestinian rabbi of Spanish origin (published in Venice, 1564), with notes inserted in the text by a Polish rabbi, also of the sixteenth century (published in Cracow, 1571), and several layers of commentaries surrounding the text, added in the seventeenth, eighteenth and nineteenth centuries. A formidable sample page of the Shulhan 'Aruk, Vilna ed., 1873, serves as frontispiece. Undismayed, and believing that the critical inspection of myriads of animals for nearly two thousand years must have yielded important anatomical observations, Dr. Boyden made light of the labor. The French version of the code by Pavly and Neviasky (1898) was found to be neither literal nor anatomically dependable. In no other modern language were the terefah sections available.

It happened that Professor George F. Moore, theologian, sometimes accounted the most learned member of the Harvard faculty, had been puzzled by the Scriptural and Talmudic references to a "redundance upon (or of) the liver," to be burnt upon the altar. Its various interpretations as omentum (caul), diaphragm or "finger" of the liver caused him to make personal inspection. All that he wrote of this λοβός in an essay (1906) with title in Hebrew, Greek, Aramaic, Arabic and Syriac appears correct, save that he identified it with "the caudate lobe" rather than "the caudate process of the caudate lobe"—a nomenclatorial error which he humbly confessed. At Dr. Boyden's request, Professor Moore called in a rabbinical scholar of Boston who reviewed for him the entire Talmudic and later Hebrew descriptions of the gall bladder, which was the stimulus for the larger project. "Eventually," writes Dr. Boyden, "it became my good fortune to enlist the aid of the senior orthodox rabbi of Minneapolis"—Rabbi S. I. Levin. Rabbi Levin undertook to translate all the 32 sections of the treatise on Terefot, with summaries of such portions of the extensive later commentaries as seemed appropriate, making the book an authentic English version of the regulations now in force, sound from the viewpoint of orthodox Jewry. With diverse but non-conflicting objectives—namely, the production of an orthodox manual and the presentation of everything of anatomical interest in this extensive literature—the joint authors collaborated until their difficult and valuable work was happily concluded by the publication of the rather small volume before us.

"The reason why slaughtering must be done at the neck and the knife must be inspected," writes Rabbi Levin in the Introduction, "is that the animal or fowl should not be made to suffer: the Lord has permitted us to use the animal or fowl, but not to cause it to suffer needlessly." Emphatically the Pentateuch enjoins, "Ye shall eat no manner of blood, whether it be of fowl or of beast—for the life of the flesh is in the blood: thou shalt pour it upon the earth as water." Accordingly, the throat must be cut, and "lost in tradition" is the source of the further precept regarding the gullet and windpipe, that "most of one of them in a fowl, and most of both of them in cattle, should be severed at the time of slaughtering."

This method of slaughter, which differs from the classical Greek and Roman in requiring a sharp smooth knife without a nick and in not allowing cattle to be felled with an axe, has been described by Dr. Isaak Dembo, of St. Petersburg, as "ideal," if that word could be applied to the killing of an animal. The cut "is entirely painless," he finds, and consciousness is lost within "three to five seconds." Dr. Dembo's approval of "The Jewish method of slaughter compared with other methods from the humanitarian, hygienic, and economic points of view" was published, in authorized translation, in London, 1894. In prefatory letters, Virchow hopes that the researches will "at last bring comfort to distressed minds," and Professor Preyer finds the Jewish method "the safest and most expeditious." Ten years later, in 1904, the British Government appointed a distinguished committee, Mr. Arthur H. Lee, Civil Lord of the Admiralty, *chairman*, "to ascertain the most humane and practicable methods of slaughtering animals for human food." Its report (Parliamentary Papers, Cd. 2150) recommends that "all animals, without exception, must be stunned, or otherwise rendered unconscious, before blood is drawn." In collaboration with two such eminent physiologists as Sir Michael Foster and Professor Starling, the committee was forced to conclude that "the Jewish system fails in the primary requirements of rapidity, freedom from unnecessary pain, and instantaneous loss of sensibility, and that it compares very unfavourably with the methods of stunning." In view of these diverse verdicts and with no comparable deliverance by American physiologists, the reviewers can perhaps agree that the slaughter-house is not yet a place of twilight sleep: improvement may not be impossible.

The Kosher Code is an intricate system of regulations and practical measures for determining whether or not an animal is healthy and fit for food. The origin of these laws is a brief statement in the Pentateuch that an animal torn by a beast of prey is not to be eaten. Out of this, these laws developed along the same lines as all the other oral laws of Judaism.

There must have been a nucleus of supplementary oral traditions existing by the side of the written law from its very inception. In course of time, with the appearance in Judaism of general principles of interpretation of the written law, the theory was evolved that the written statement of the Mosaic law should not be taken in a strict and narrow literal sense as a prohibition to eat the meat of an animal that has been actually torn by a beast of prey, but should rather be understood in a more general way as prohibiting the meat of any animal that has been mortally wounded or ravaged by some disease. Since the marauder *punctures* with his claws, the Mosaic law was interpreted as prohibiting all similar perforations, whether by needle, thorn or disease; and difficult decisions must be made when the apertures are closed by adhesions or scar tissue. The neat round holes in the wall of the aorta are orifices of intercostal branches and can be disregarded. "If the heart is perforated, and the hole does not reach the cavity, the animal is kasher if the perforation is due to disease; but if it is made by a thorn or needle, it is terefah."

Predatory animals tear out organs; and missing parts, whether from disease or congenital absence, may disqualify. So also the anomalous doubling of certain organs, on the principle that "every addition is considered an absence"; but always there are exceptions. In kosher animals the spleen or kidneys may be absent; or two spleens or three kidneys may be present. Some rulings, perhaps, depend on hypothetical cases. The finest description of normal and diseased bovine lungs in antiquity is prominently presented. The lungs, indeed, receive special attention: they should be inflated in searching for adhesions and solidifications, but with more fear of punctures than of tubercles. Without discriminating clearly between small and large intestines, Meckel's diverticulum is evidently recorded. Aristotle's *echinus* receives its current name *omasum*.

Thus the free interpretation of the Mosaic law has led to the study and observation of the animal body, its structure and its functions; and religion, here as elsewhere, thus became indirectly the incentive of scientific observations and generalizations. But the scientific interest was subservient to the religious interest and its scope was limited by the special religious needs. In the literature produced in Judaism between the second and fifth centuries after the Christian era, *i.e.*, in the Mishnah and Talmud, the body of accumulated scientific observations and notions, together with their practical application, was committed to writing, and from that time on it remained substantially unchanged.

The medical conceptions in this literature, as has been observed by Professor Boyden, approach those of the Hippocratic school, though, it must be added, there is no evidence that they have been formed under the direct influence of Greek medical literature. They undoubtedly represent a native development, under the exigencies of religion, of certain rudimentary elements inherited from the ancient Semitic world and perhaps also of certain elements absorbed from the later Hellenistic environment.

But in the meantime the science of the structure and functions of the animal body grew apace independently of religion, and the results of the findings of this independent science are not always in agreement with the traditional religious science. To the student of religious institutions the question may, therefore, occur whether it would be possible in Judaism to replace the old traditional science by the new science to be used in the service of its laws with regard to *terefah*. The question was actually raised in the twelfth century by Maimonides, who, besides being a great codifier of Jewish law, was also a great philosopher and scientist, and was especially acquainted with the writings of Galen and the Arabic medical literature. It is interesting to note that, while in the purely speculative elements of religion Maimonides did not hesitate to interpret the most fundamental beliefs of traditional Judaism in terms of what he believed to be the true scientific principles of philosophy, in its practical observances relating to the laws of *terefah* he openly expressed himself in opposition to replacing the old traditional regulations by the new findings of science. In Judaism, as in any other religion, there is always a greater plasticity in abstract ideas than in concrete observances and institutions. The code under consideration, therefore, though written in the sixteenth century, represents the early native Jewish medicine and does not reflect the Greek and Arabic medical knowledge which by the time of its writing had already been accessible in Hebrew translations, as well as in original Hebrew works, for about three centuries.

It would be, however, wrong to assume that the observance of the orthodox Jewish laws of *terefah* would ever come into actual conflict with modern regulations concerning the same matters. With all its insistence upon the traditional methods of determining the health of animals, orthodox Judaism can have no objection, on purely religious grounds, to have those methods supplemented by modern scientific methods, whenever the former methods prove to be scientifically inadequate and ineffective.

Much more might be said. A code of laws is seldom entertaining literature. We have here no masterpiece of prose, but a source-book of extraordinary interest. We commend it to the consideration of the Book of the Decade Club.

HARRY A. WOLFSON
FREDERIC T. LEWIS

HARVARD UNIVERSITY

REPORTS

RESEARCH WORK ON CHEMOTHERAPY

AT the meeting of the Division of Chemistry and Chemical Technology of the National Research Council in Washington, D. C., in April, 1939, the opinion was expressed that research work should be stimulated in the field of quinine substitutes and synthetic antimalarials.

While quinine, plasmoquine, atebrine and a few other drugs have been useful, there is in the judgment of the medical profession, great need for something better. The present remedies leave much to be desired from a therapeutic point of view, and the price is higher than many can afford to pay. Further, so far as quinine itself is concerned, the world is practically dependent for its supply on Java and the Kina Bureau (its selling agent) and might be cut off from this supply or perhaps compelled to pay exorbitant prices if Java should be seized by some other nation. The trend is therefore toward synthetics as the direction which holds out the best promise of new and useful antimalarials.

Contrary to popular belief, malaria is still one of humanity's major scourges. India, with a population of some 320,000,000, has a yearly average of from 70 to 80 million sufferers, and President Fosdick, of the Rockefeller Foundation, in his recent annual report, has called attention to the threatening situation in South America. In certain tropical countries the incidence of this disease is 100 per cent. Throughout the world it is stated to be the direct cause of approximately a million deaths annually.

Dr. Herbert R. Moody, chairman of the division, was duly empowered to appoint a temporary committee to survey the field of malaria and antimalarials and to report back to the division whether or not it would be advisable to designate a regular standing committee of the division to study, in cooperation with the division of medical sciences, the whole problem of the chemistry and synthesis of antimalarials.

The temporary committee appointed by Dr. Moody, from the division of chemistry and chemical technology, consisted of Dr. Leonard H. Cretcher, assistant director of the Mellon Institute of Industrial Research, Pittsburgh; Dr. Lyndon F. Small, head chemist of the National Institute of Health, U. S. Public Health Service, Washington, D. C., and Marston T. Bogert, emeritus professor of organic chemistry in residence, Columbia University, New York, as chairman.

Later, on the nomination of Dr. Esmond R. Long, chairman of the National Research Council division of medical sciences, the following members for that division were added to the temporary committee: Dr. Lowell T. Coggeshall, International Health Division, Rockefeller Foundation, New York, and Dr. Torald H. Sollmann, dean, Western Reserve University School of Medicine, Cleveland.

The completed committee thus consisted of the following chemical and medical representatives: Lowell T. Coggeshall, Leonard H. Cretcher, Lyndon F. Small, Torald H. Sollmann and Marston T. Bogert, chairman.

As a result of this preliminary survey, the temporary committee reported unanimously that, in its judgment, a regular standing committee of the division of chemistry and chemical technology should be constituted by the National Research Council, to concern itself not solely with antimalarials, but whose function should be chemistry in the service of medicine and its title Committee on Chemotherapy, with antimalarials as its first assignment.

This broadening of the field to be covered by the committee will not reduce in any way its usefulness in an attack on the malaria problem but will permit it to offer its services in attempts to combat other diseases as well, particularly in those cases in which acute emergencies may arise and our help be welcome. Even in the investigation of antimalarials, other diseases are concerned, for example, the possible use of cinchona alkaloids or their derivatives in the treatment of pneumonia, a problem which has been under investigation by Dr. L. H. Cretcher at the Mellon Institute for a number of years.

At the meeting of the division of chemistry and chemical technology, National Research Council, November 18, 1939, in Washington, D. C., the report of the temporary committee was presented and adopted and the committee was discharged. The division then recommended that the same personnel be appointed as one of its regular standing committees, to be known as its Committee on Chemotherapy. This recommendation of the division was subsequently approved by the National Research Council, and Surgeon General Thomas Parran, Jr., of the U. S. Public Health Service, gave his prompt consent to the service of Dr. Small on the committee.

The Academy of Tropical Medicine, the American Society of Tropical Medicine and the National Malaria Committee met at Memphis, Tennessee, November 21–24, 1939, and unanimously endorsed the program of the committee. The council of the American Academy of Tropical Medicine also expressed its cordial approval of the selection of Dr. L. T. Coggeshall as a member of the committee.

The Symposium on Malaria, May 29–30, 1940, in Atlanta, Ga., called by Surgeon General Parran, for the purpose of a preliminary discussion of the malaria situation in our country, with special emphasis on chemotherapy, was attended by Drs. Coggeshall and Small of the committee.

Section N of the American Association for the Advancement of Science, at its Philadelphia meeting, December 27, 1940, to January 2, 1941, will hold a symposium on the human malarias, in which members of the committee will participate. The proceedings of this symposium when published will constitute an important review of present knowledge of this disease.

Active cooperation has been promised by an extensive list of educational and research institutions, scientific and technical organizations, manufacturers and individual scientific investigators, including the following:

Advisory Commission to the Council of National Defense, Edward R. Weidlein, executive, for Industrial Division C, Chemical and Allied Products, Industrial Materials Department (Edward R. Stettinius, Jr., commission member).

American Association for the Advancement of Science, Malcolm H. Soule, secretary of Section on Medical Sciences (N).

American Chemical Society, Divisions of Biological Chemistry, of Medicinal Chemistry and of Organic Chemistry.

American Drug Manufacturers Association, Carson P. Frailey, executive vice-president and secretary.

Army and Navy Munitions Board, Warren N. Watson, secretary, Chemical Advisory Committee.

Cinchona Products Institute, Norman Taylor, director.

Harvard University Committee on Pharmacotherapy, Soma Weiss, chairman.

Institut Pasteur d'Algérie, Algiers, Edmond Sergent, director.

Manufacturing Chemists Association, Warren N. Watson, secretary, and George W. Merck, chairman of Medicinal Chemicals Section.

National Conference on Pharmaceutical Research, John C. Krantz, Jr., secretary.

National Research Council Division of Medical Sciences, Lewis H. Weed, chairman, and Perrin H. Long, chairman of its Committee on Chemotherapeutic and Other Agents.

National Research Council Survey of Research in Industry, F. W. Willard, chairman.

School of Tropical Medicine, San Juan, Puerto Rico, George W. Bachman, director.

Tennessee Valley Authority, F. L. Roberts, University of Tennessee.

U. S. Department of Commerce, C. C. Concannon, chief, Chemical Division, Bureau of Foreign and Domestic Commerce.

U. S. Department of the Navy, Rear Admiral Ross T. McIntire, Surgeon General.

U. S. Public Health Service, Thomas Parran, Jr., Surgeon General.

U. S. War Department, Major General James C. Magee, Surgeon General.

The objects of the committee include:

1. Chemistry in the service of medicine, with special emphasis on the discovery of new and useful synthetic drugs.

2. To bring about a closer cooperation between chemists and pharmacologists interested in the same field. It often happens that an organic chemist synthesizes a new compound of therapeutic possibilities but does not know to whom to turn to have it tested. Similarly, a pharmacologist discovers that a certain chemical exhibits hitherto unsuspected physiologic effects but has no information as to which organic chemists could help him in the preparation of the compound and its more promising derivatives.

3. To help in this direction, the committee should function as a general clearing house for the collection and classification of information as to the chemists, manufacturers, pharmacologists and others in the United States who are now at work in this field, the special lines of investigation they are following and any other pertinent data. With this information in hand, and that accumulated last summer by the survey of research resources of educational institutions, in men and material, now classified and on file in the offices of the division of chemistry and chemical technology of the National Research Council in Washington, we should be able more intelligently to allocate and evaluate the various research problems to be investigated. In this way we can also be of some assistance in reducing the amount of unintentional duplication and overlapping in research work and suggest important problems for investigation where the need seems clearly indicated.

4. Not to undertake too ambitious a program at the outset, since the field of chemotherapy is a vast one. For the reasons explained in the foregoing pages, malaria has been selected as the first disease to be attacked.

5. To prepare a compact semi-popular presentation of the malaria situation throughout the world, particularly in our own land, including a summary of what is going on here and how it is retarded and handicapped by lack of funds, for the purpose of enlisting public appreciation and support.

6. To secure funds from interested individuals, institutions, foundations, firms and others for the support of the work of the committee, the establishment of research fellowships and such other activities as will advance and expedite the achievement of the results sought.

Such a survey is now being conducted to ascertain what investigations are under way or planned for the synthesis of new and useful antimalarials, to compile a list of individuals and firms interested in this field, just what they are doing and (as far as may be possible) in what other directions they are arranging to expand.

Simultaneously, bibliographies, reviews and digests of the literature covering various aspects of the problem are being compiled, but all this work can progress

but slowly under existing limitations, and speed is urgently needed; for the medical profession confidently predicts a malaria peak this year or next.

Those individuals, organizations, institutions and manufacturers who are willing to cooperate by supplying the information suggested in the foregoing pages are requested to communicate with the chairman of the committee, whose address until September 20 will be Belgrade Lakes, Maine; after that, Havemeyer Hall, Columbia University, New York.

As this committee has been for some time in touch with many others interested in antimalarials both in this and in other countries and has accumulated considerable information of value, it seems to us wise to publish this statement, in order that confusion and duplication of effort may be avoided and national preparedness advanced more speedily, for we have been informed that other committees and groups have been or are about to be organized to cover much the same field, evidently unaware of the existence of our committee and its activities.

LOWELL T. COGGESHALL
LEONARD H. CRETCHER
LYNDON F. SMALL
TORALD H. SOLLMANN
MARSTON T. BOGERT, *Chairman*

SPECIAL ARTICLES

THE ISOLATION IN PURE FORM OF THE INTERSTITIAL CELL-STIMULATING (LUTEINIZING) HORMONE OF THE ANTERIOR LOBE OF THE PITUITARY GLAND

SHORTLY after proof of the secretion of gonadotrophic hormones by the anterior lobe of the pituitary body had been offered by the work of Smith,[1] Zondek and Aschheim,[2] and a number of later investigators,[3] it was suggested that the effects of implants or crude extracts of the pituitary on the gonads should be attributed to at least two hormones (follicle-stimulating and luteinizing hormones). This view-point was supported by Fevold, Hisaw and their collaborators and, more recently, by work in this laboratory and in that of H. M. Evans. However, some investigators are not satisfied that more than one gonadotrophic hormone can be extracted from the gland and have awaited the isolation of pure hormones which should satisfactorily correct the existing confusion with regard to the possible number and effects of the gonadotrophic hormones postulated by others. In New Brunswick,[4] considerable progress had been made in isolating a hormone[5] stimulating the interstitial cells of the male or female gonads. This material was not chemically pure but was found to be free of other pituitary hormones. The work here reported describes the isolation of this hormone as a pure protein.

[1] P. E. Smith, *Anat. Rec.*, 32: 221, 1926.
[2] B. Zondek and S. Aschheim, *Klin. Wschr.*, 6: 248, 1927.
[3] H. B. van Dyke, "The Physiology and Pharmacology of the Pituitary Body," Chicago, I, 1936; II, 1939.
[4] R. O. Greep, H. B. van Dyke and B. F. Chow, *Jour. Biol. Chem.*, 133: 289, 1940.
[5] Evans, Korpi, Simpson, Pencharz and Wonder (*Univ. Calif. Pub. Anat.*, 1: 255, 1936) isolated an interstitial cell-stimulating fraction which was considered not to cause luteinization. Later, Evans, Simpson, Tolksdorf and Jensen (*Endocrinology*, 25: 529, 1939) concluded that the same fraction can bring about both interstitial cell-stimulation and luteinization.

The initial extraction of gonadotrophic hormones from the pituitary glands of swine was undertaken by a method already described.[6] The interstitial cell-stimulating extract free from other hormones was then isolated by a second procedure.[4] Electrophoretic studies were carried out on this preparation in a Tiselius apparatus,[7] using the scanning method of Longsworth[8] for obtaining the electrophoretic patterns. Experiments were performed over a range of pH values from about 4.5 to 8.0 at a constant ionic strength of 0.05, using the monovalent buffers, acetate, cacodylate and diethylbarbiturate. The results showed the presence of three components (Fig. 1 A). After

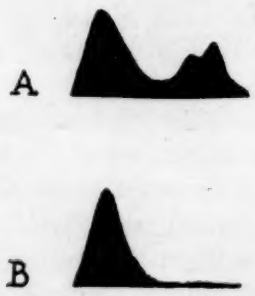

FIG. 1. Electrophoretic patterns at pH = 7.85. A—Before final purification. B—After final purification.

sufficient separation of the electrophoretic boundaries it was possible to isolate two fractions, one containing the major component, the other the two minor ones. The biological activity was found to be entirely in the main component. A plot of mobility against pH showed the isoelectric point of this main component to be at pH 7.45. The isoelectric points of the contaminating, inert components fell between pH 4 and 5.

This information made it possible to secure the pure hormone by isoelectric precipitation. To a concen-

[6] B. F. Chow, R. O. Greep and H. B. van Dyke, *Jour. Endocrinol.*, 1: 439, 1939.
[7] A. Tiselius, *Tr. Faraday Soc.*, 33: 524, 1937.
[8] L. G. Longsworth, *Jour. Am. Chem. Soc.*, 61: 529, 1939.

trated solution of the material thus studied, ammonium sulfate was added to one third saturation and the pH was adjusted to 7.3–7.4, as measured by the glass electrode. The precipitate, after the separation of the solvent by centrifugation and the removal of the solvent, was dissolved in water; saturated ammonium sulfate solution equal to one half of the volume of water used was added, and the pH was again adjusted to 7.3–7.4. These steps were repeated seven times, and the final precipitate was dissolved in a small volume of water; the solution was then dialyzed free from ammonium sulfate. (The supernatant fluids contained considerable amounts of hormone and were set aside for further recovery.) The product obtained appeared as a single component (Fig. 1 B) in electrophoretic analysis of solutions having different pH values, with mobilities of -3.85×10^{-5} at pH = 4.58, -2.01×10^{-5} at pH = 6.21, and 0.66×10^{-5} at pH = 7.86. The material was found to show the phenomenon of "reversible boundary spreading" which is usually observed with globulins. That is, after electrolyzing for a certain length of time, reversal of the current results in a sharpening of the boundary. This indicates that the spreading of the boundary observed with increased time of electrolysis is not due entirely to diffusion but results from the fact that the material consists of molecules corresponding to a range of mobility within relatively narrow limits. If one takes the view that native proteins exist as folded molecules, such mobility ranges may be due to minor variations in folding corresponding to little or no energy differences, but yielding slight variations in the surface charge distributions which in turn result in corresponding mobility variations.

Preparations of the hormone in different degrees of purity were also studied in the ultracentrifuge. The apparatus used was of the air turbine drive type described by J. Bauer and E. Pickels.[9] The sedimentation rate was followed by the scale method of Lamm and the "schlieren" method of Philpot as modified by Svensson. Both methods gave identical results. The solvent commonly used was an aqueous solution of 1 per cent. or 0.5 per cent. sodium chloride. In one case the solvent was a cacodylate solution (pH = 6.2) which was used in the electrophoretic studies. Seven runs were made on preparations of different degrees of purity. The least pure preparation, in which three components were detected in electrophoretic studies, showed four well-separated boundaries. Two of the impurities sedimented at a faster rate than the biologically active main component (the fastest at three times the rate), whereas the third impurity sedimented at one half the rate of the hormone. A "schlieren" diagram of the sedimentation of the pure hormone can be seen in Fig. 2. The diagram represents the positions of the boundary moving from right to left at half hour intervals under a mean field of 2.37×10^{8} cm/sec.[2]

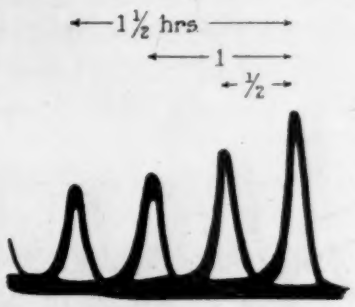

FIG. 2. Sedimentation patterns of the hormone after final purification.

(57600 r.p.m.). The symmetry of the curves and the absence of any displacement of the base line with time indicate the purity of the hormone in agreement with electrophoretic patterns and solubility tests.[10] The average sedimentation constant in a 1 per cent. sodium chloride solution was $S_{20} = 5.39\times10^{-13}$, the maximum deviation from this average value being ± 1 per cent. (An abnormally high value of $S_{20} = 6.65\times10^{-13}$ was found for the cacodylate solution.) The diffusion constant determined in the cacodylate medium was found to be $D^{20} \cong 5.9\times10^{-7}$, this value decreasing with time. From these data the molecular weight may be estimated to be approximately 90,000, assuming the value 0.749 for the specific volume of the hormone.

An example of the results of a study of the solubility of the hormone is shown in Fig. 3. The solvent

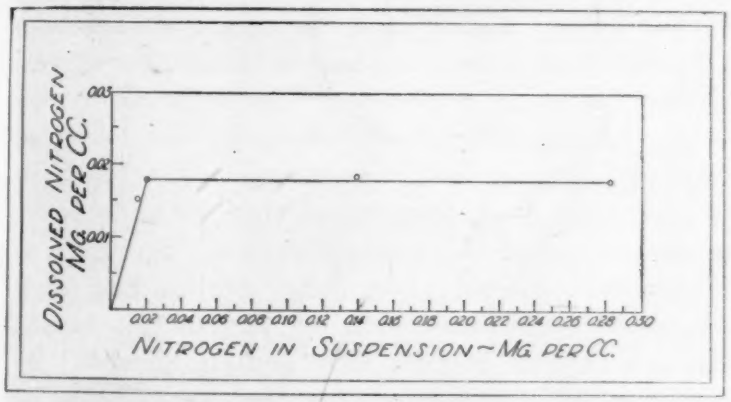

FIG. 3. Solubility of the hormone in 0.25M acetate buffer, pH = 4.35, containing 21.05 per cent. Na_2SO_4.

used was 0.25 M acetate buffer, pH = 4.35, containing 21.05 per cent. sodium sulfate. The solution remained water clear after the first addition of hormone (0.015 mg N per cc), but became turbid when 0.020 mg of N was present in each cc of solvent. The solubility then and thereafter was constant (0.018 mg N per cc), although the amount of hormone suspended in the solvent was nearly sixteen times the saturating concentration. Therefore, within the described limits, only

[9] T. Svedberg, "The Ultracentrifuge," Oxford, 1939.

[10] Close inspection of the plate revealed a faint residual trace of the impurity sedimenting slightly faster than the hormone.

one component was demonstrated, since the solubility of this component was constant whether the solution was just turbid or whether a great excess of hormone was in suspension. These results, agreeing with the studies made with the Tiselius electrophoresis apparatus and the ultracentrifuge, furnish proof of the purity of the hormone by the only other available physical chemical test.

The results of analysis of one specimen of pure hormone were: carbon, 49.37 per cent.; hydrogen, 6.83 per cent.; nitrogen, 14.93 per cent.; ash, 0.93 per cent.

All the assays were performed in rats hypophysectomized at an age of twenty-one days. Injections were begun two days later and were given subcutaneously once daily for four days.[11] At necropsy, twenty-four hours after the last injection, the appropriate organs were weighed and then fixed in Bouin's fluid. Typical results are shown in Table 1. Owing

TABLE 1

Dose micrograms of nitrogen	Number of rats	Mean and S.E. of wts.		
		Body gm	Ant. lobe prostate mg	Testes mg
–	8	48.5 ± 2.90	8.49 ± 0.549	130.7 ± 5.25
1	9	52.4 ± 2.49	11.01 ± 0.695	142.0 ± 7.35
2	6	49.3 ± 3.62	16.97 ± 0.624	160.8 ± 12.10
10	5	44.9 ± 1.18	34.58 ± 2.130	209.6 ± 7.91

to stimulation of the testicular interstitial cells, 1 microgram of hormone nitrogen caused a significant increase in the fresh weight of the anterior lobe of the prostate ($P = < 0.05$ by Fisher's method of paired comparisons). A dose of 2 micrograms of hormone nitrogen was required to produce an increase of testicular weight ($P = < 0.05$). This testicular hypertrophy was probably caused chiefly by the action of secreted androgen on the germinal epithelium. In hypophysectomized, immature female rats, the hormone maintained the interstitial cells. However, if it was administered after follicle-growth had been stimulated by follicle-stimulating hormone, it caused the formation of corpora lutea perhaps preceded by ovulation. Our results indicated that the secretion of oestrogen did not occur following the injection of follicle-stimulating hormone into hypophysectomized rats, although the growth of histologically normal follicles was clearly present. If, however, interstitial cell-stimulating hormone was also administered, all the morphological phenomena of oestrus were provoked.

Summary

Interstitial cell-stimulating (luteinizing) hormone was isolated from swine pituitary glands. This protein hormone, having a molecular weight of about 90,000 and an isoelectric point of pH 7.45, was shown to be pure by tests in the electrophoresis apparatus of Tiselius and in the ultracentrifuge as well as by its constant solubility. The hormone stimulated the interstitial tissue of the testis or ovary and caused the formation of corpora lutea provided that maturing follicles were present. Under the conditions described, its minimal effective total dose in hypophysectomized immature male rats was about 1 microgram of nitrogen or 6.7 micrograms of hormone. Extracts of swine pituitary with purely follicle-stimulating effects did not cause oestrus in immature hypophysectomized female rats unless luteinizing hormone was also administered.

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SYNTHESIS OF THE PYRIDINE ANALOG OF VITAMIN B_1

THE concept of isosterism as proposed by Grimm[1] and extended by Erlenmeyer[2] has recently attracted attention by its possible application with respect to the substitution of a pyridine nucleus for the thiazole component of the thiamine (vitamin B_1) molecule. As part of a general study of various pyridine derivatives in this laboratory, one of us[3] recently described the preparation of 2-(β-hydroxyethyl)-3-methylpyridine and the condensation of this substance with the pyrimidine component of thiamine. Meanwhile, others have reported the synthesis of what was assumed to be 2-methyl-3-(β-hydroxyethyl)-pyridine, and from this the preparation of a substance presumably isosteric with thiamine by condensation with the appropriate pyrimidine. Schmelkes[4] described an isosteric vitamin as well as the substances formed by condensation of what he believed to be 2-methyl-5-(β-hydroxyethyl)-pyridine with the thiamine pyrimidine. He reported anti-polyneuritic activity for the former. Baumgarten and Dornow[5] arrived at Schmelkes' antipolyneuritic substance by a different route and reported its activity as being about one-twenty-sixth that of thiamine. However, in a later paper, Dor-

[11] Results of other investigators indicate that intraperitoneal injection would have provoked a greater response.

[1] Grimm, *Naturwiss.*, 17: 535, 557, 1929.

[2] Erlenmeyer *et al.*, *Helv. Chim. Acta*, 16: 733, 1381 (1933); 20: 1388 (1937) inter alia.

[3] Finkelstein and Elderfield, *Jour. Org. Chem.*, 4: 365, 1939.

[4] Schmelkes, SCIENCE, 90: 113, 1939; Schmelkes and Joiner, *Jour. Am. Chem. Soc.*, 61: 2562, 1939.

[5] Baumgarten and Dornow, *Ber.*, 73: 44, 1940.

now[6] showed that the assumed β-hydroxyethylpyridine derivative of both workers was in reality the α-hydroxyethyl derivative. The true isoster of thiamine has, therefore, not been described, and we wish to report its synthesis at this time.

Ethyl α-(β-ethoxyethyl)-β-aminocrotonate condenses smoothly with diethyl malonate in the presence of sodium ethoxide[7] to give 2-methyl-3-(β-ethoxyethyl)-5-carbethoxy-4,6-dihydroxypyridine. Elimination of the carbethoxy group in this substance by saponification and decarboxylation and of the hydroxyl groups by replacement by chlorine and subsequent catalytic reduction of the dichloropyridine gave 2-methyl-3-(β-ethoxyethyl)-pyridine (picrate, m. 63–64°; calculated for $C_{16}H_{18}O_8N_4$: C, 48.7; H, 4.6; found: C, 49.0; H, 4.4). Cleavage of the ether group in this compound gave 2-methyl-3-(β-hydroxyethyl)-pyridine, the position of the substituents in which was shown by oxidation to quinolinic acid. The pyridine crystallizes with one water from chloroform-petroleum ether, and the water is not lost on distillation at 0.5 mm. pressure. The hydrate melted at 61–62° (calculated for $C_8H_{11}ON \cdot H_2O$: C, 61.9; H, 8.5; found: C, 62.2; H, 8.8). The picrate melted at 123–124° (calculated for $C_{14}H_{14}O_8N$: C, 45.9; H, 3.9; found: C, 46.3; H, 4.1). The above pyridine was condensed with 2-methyl-5-bromomethyl-6-aminopyrimidine hydrobromide to give 1-[(4-amino-2-methyl)-5-pyrimidyl-methyl]-2-methyl-3-(β-hydroxyethyl)-pyridinium bromide hydrobromide, the true pyridine analog of thiamine, which charred at 240–260° dec. (calculated for $C_{14}H_{20}Br_2N_4O.H_2O$: C, 38.4; H, 5.1; found: C, 38.5; H, 4.6).

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[6] Dornow, *Ber.*, 73: 353, 1940.
[7] Knoevenagel and Fries, *Ber.*, 31: 767, 1898.

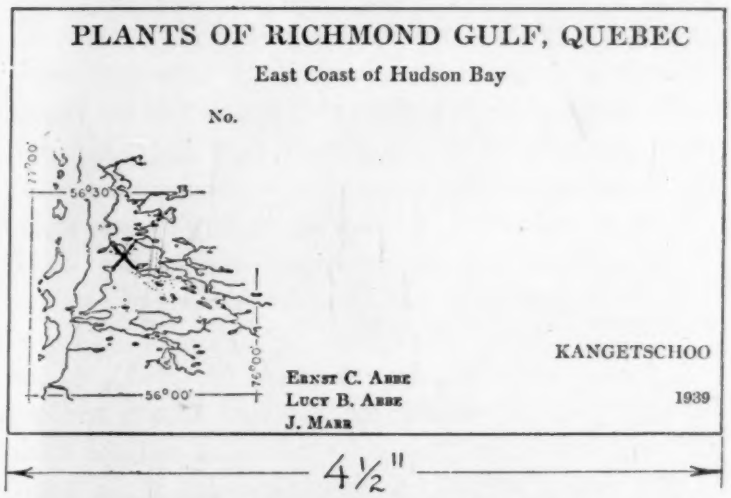
SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE USE OF OUTLINE MAPS ON HERBARIUM LABELS

THOSE who have had occasion to use herbarium specimens in distributional studies have experienced the aggravations attendant upon unsatisfactory definition of the localities listed on the labels. The problem affects not only ecological research and floristic plant geography, but also monographic taxonomy. In the experimental fields adjacent to taxonomy it is especially desirable to be able to return to the localities represented by herbarium specimens.

In regions which are well mapped, current practice often partially fulfils the above requirement through the indication of locality by township and range, by county or by latitude and longitude. Even greater precision would be desirable for some studies, such as in ecology and the genetical aspects of ecology and taxonomy.

For plants from regions which are poorly mapped or for which accurate maps are not generally available, such as that visited by the University of Minnesota Expedition to Hudson Bay,[1] there may be recommended the type of label illustrated in Fig. 1. The outstanding difference between this and the conventional labels is the inclusion of an outline map of the area, which results in a slightly larger than average size label (as indicated in the figure). There still remains generous space on the herbarium sheet for the plants and abundant space on the label for further data. The indication of the specific location at which the specimen was collected merely involves the insertion of a symbol (shown with an X in the figure) in the appropriate place on the outline map on the label. The added cost of this type of label is primarily accounted for by the cost of the zinc etching—in this case (at local prices) about $1.25. Thus the added cost per thousand labels is negligible.



The objection might be raised that a simple indication of latitude and longitude would be sufficient; but in poorly mapped regions an area may be very inadequately located with respect to the parallels of latitude and longitude. Furthermore, such regions are seldom shown in adequate detail even in the best atlases.

The locations of place names not given on standard maps and of places wholly lacking accepted names but to which temporary or local names have been given are clarified through the use of a map on the label. Such place names *may* be elucidated in diaries or in field note-books or in general accounts of collecting trips. Too frequently, however, these sources are not available to, or known by workers in the herbaria to

[1] Abbe, SCIENCE, n. s., 90: 458, 1939.

which the specimens finally find their way. Frequently, too, such scattered and obscure sources of vital information become lost, and occasionally the place names used seem never to have existed except in the mind of the collector.

Since the method here described leads to precision in floristic studies and involves only slight additional cost in time and money over the conventional method of labeling, it should find ready use not only in the field of botany but in any field where precision in recording the source locality of natural objects is of importance.

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A SPATULATE PIPETTE SERVING AS SECTION LIFTER

DOUBTLESS one of the most distressing manipulations in the histo-pathological laboratory is the handling of frozen sections of tissue or of transferring celloidin sections from fluid to fluid. In the first case, a partial or total loss of a section is the rule rather than the exception. One reason for such high casualty is the want of suitable tissue carriers. The most commonly used carrier is a glass rod, but to this the tissue frequently adheres so completely that destruction of the section during the attempt to loosen it and spread it out is unavoidable. Some workers employ a spatula, a micro-slide or a brush, and though such appear to be preferable to the glass rod, considerable loss of sections is admitted.

A former associate, Dr. Baeslack (d. 1929), at the University of Würzburg, manipulated frozen sections with pipettes made from glass tubing of various sizes. In this way a section remains suspended in its liquid medium (water, alcohol, dye, etc.) while being transferred, and consequently the possibility of the loss of the section is reduced to a minimum, since it is not touched mechanically until it is placed on the slide. But a tube with a round lumen encourages the frequent twisting and folding of the section, making it difficult to flatten it. The writer overcame this disadvantage by making a flattened pipette, as illustrated in Fig. 1. The device is published here at the suggestion of Professor Kampmeier, who was impressed by its simplicity and effectiveness. Its usefulness as a section "lifter" or "carrier" has been demonstrated sufficiently well in our laboratory to bring it to the attention of other workers.

To make a spatulate pipette, one end of a thin-walled glass tubing with an inner diameter of about 7 mm (approx. ¼ inch) is heated in the flame to plasticity and placed between asbestos-covered jaws of a pair of pliers. To prevent cracking of the glass tube as the jaws are gently squeezed down on it, the asbestos must of course also be hot. With a little practice a uniformly flat chamber is made from suitable glass tubing, the size of the opening or slit being varied according to need; a slit 1 cm long and 2 mm in height

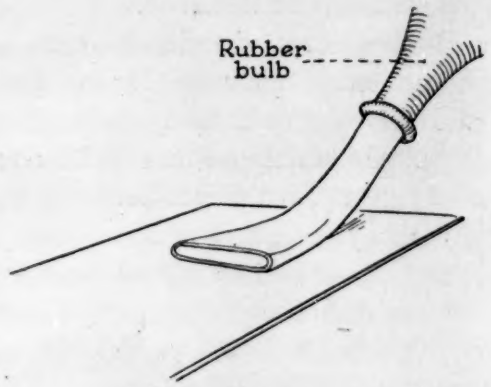

FIG. 1.

or width is generally adequate. The tube is then slightly bent, as shown in Fig. 1, to make it handier. A rubber bulb, as used on eye-droppers, is attached to the round end of the tube to complete the instrument. It is imperative that the pipette is always clean to prevent the sticking of the section to the glass as it is drawn into the lumen. For similar reasons any sharp edges at this end of the pipette must be rounded in the flame.

It is hardly necessary to add directions regarding the use of the spatulate pipette, so obvious are its advantages. When transferring a frozen section, for example, to a slide, it is allowed to be drawn into the slit of the pipette slowly and in a flattened condition with the fluid in which it is suspended. The middle of the slide is then flooded with a little additional fluid and the section expelled under gentle pressure so that it floats on the pool on the slide. The excess fluid is drained or sucked off with the pipette, letting the section settle flat on the slide. Should the section be crumpled or folded in part, fluid is rushed under that part which needs correction, meanwhile tilting the slide downward in the same direction; this maneuver prevents the section from separating from the slide.

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HOLMAN, RICHARD M. and WILFRED W. ROBBINS. *Elements of Botany.* Third edition. Pp. xi + 392. 277 figures. Wiley. $2.75.

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